Solms Laubach

Monsieur!

En désirant d'étendre mes travaux sur la biologie des Rafflesiacées j'avais écrit à Mr Torrey pour obtenir de sa bonté s'il était possible un echantillon du Filoxylon Thurberi qu'il a décrit dans son mémoire sur les plantes de l'exploration de Mon. le colonel Emory. Je m'adresse maintenant à vous Monsieur en n'ayant point reçu de réponse du Dr Torrey pour l'avoir par voie que j'aurai grande peine s'il me serait effectivement impossible de l'étudier. Il me suffirait pour

mon étude la moindre branche du Salix avec le parasite. Comme il serait possible que les plantes de Mr. Thurber n'étant pas récoltées en très grand nombre ne seraient pas en possession de Mr. Torrey et des différents musées américains mais qu'elles feraient partie de certaine collection de l'État à Washington New York ou Philadelphie je vous serai très reconnaissant si vous voudriez bien me faire parvenir des dates exacts là dessus, en m'indiquant la manière pour ~~avoir~~ prêter des échantillons de la collection en question. J'ose espérer même que votre influence dans les musées américains ~~voudra~~ bien m'aider en ce que je demande. Je vous promets d'abord naturellement de renvoyer l'échantillon que l'on voudrait me confier le plus tôt possible et vous y causer des destructions.

Je vous adresse avec ces lignes un petit travail sur la distribution de l'oxalate de chaux dans les membranes cellulaires.

En espérant que par votre vaillant appui j'obtiendrai ce que je désire je signe

comme votre très dévoué

Comte de Solms-Laubach

Halle/Saale 12/11 71.

Truckee, May 27th 1890

Prof. S. Watson.

Cambridge, Mass

Dear Sir.

I have the 2nd part of Wright's Syn Flora & have been trying to get the supplement through Sereno, Blakman & Co without success. They say you may be able to send it to me — if so please do so & I would also like to get your ~~cost~~ contribution to Proc. Amer Acad S. & A. issued Jan 31. 1889, Vol XXI. —

If you can send me them please let me know how much they with am & I shall send a Money-order for the amount.

Yours truly

C. F. Sonne.

E. J. BRICKELL, W. H. KRUGER.

TRUCKEE LUMBER CO.,

Manufacturers and Dealers in

ROUGH AND DRESSED LUMBER.

WINDOWS, DOORS,

BLINDS, MOULDINGS,

PICKETS, SHINGLES.

Fruit and Packing Boxes.

Truckee, Cal., Nov 3d 188.

N. S. Watson,

Cambridge,

Mass.

Dear Sir.

I send you by Mail
2 specimens of Data & one of
Nevisana, which I would be obliged
if you would name for me — the
name on the tickets are the ones
I have been distributing them under
but dont know how correct I have
been. —

With thanks I am

Yours truly

C. A. Stone.

W.H. KRUGER, Pres. & Genl. Manager.
C.F. SONNE, Secty.

Office of
Truckee Lumber Co.
Incorporated 1886

Truckee. Jany 2d 1894

Prof. S. Watson.

Cambridge, Mass

Dear Sir.

I send you to-night by mail a small package of plants — 28 all told mostly Lyguminosæ (?) Astragalus & Lupinus. — Some I have had name for and some I haven't & I would be under obligations to you if you would look them over and send me the name as far as it be possible — as I know some of them are not as perfect as they should be especially the Astragalus & Lupinus. — You know when a fellow is away from home & finds up a thing it is not always very easy to come back, later to get fruit or seed of what you may get & so in my case. —

The Compositalon I found this year (I don't know whether the plant has been collected since Mr. Lemmon found it years ago) and send for the reason that it seems to me that Dr. Gray some place has united it (A. Douglasii) with the other one: A. spinosum — It seems to me I have seen it somewhere, but I may be mistaken. —

The specimens all are in good order & have mostly

W.H. KRUGER, Pres. & Genl. Manager.
C.F. SONNE, Secty.

Office of
Truckee Lumber Co.
Incorporated 1886

Truckee, _____ 189_

all retained their natural colour. — The Lupinus from
Mt. Stanford with small pale blue flowers I have collected
for you & have been told that it is L. mesonanthus, but
I have come to the conclusion it is not, but what it is
I cant tell — It grows ~~from~~ in numerous simple stems
from a perennial root. —

I inclose herewith Postal Note for $1.00 for the
supplement to the Syn. Flora, which you sent me
last spring. — I have mislaid the letter & it may have
been mine — If so please let me know & I shall
send you the balance. — I can't find my last memorandum
it has entirely slipped my memory — I send at
the same time your latest contribution to the Proc. A.S.P.A
for which please accept my thanks — Could you
spare me a copy of the last one issued? I am
desirous of having them all & if it is I will get them —
am willing to pay for them. —

Yours truly
C.F. Sonne.

Dear Doctor;

Will you — or Madame — inform Graceless, that is, Merello's man, to resume his visits to my room — in respect of bread? I am tired of that scientific contrivance yclept ἠερωμένος ἄρτος, alias Aërated bread. Had Horsford any hand in that invention?

Yours ever
E. A. Sophocles

Sept. 10, 1863, New Style.

Jan. 13, 1878.

My Dear Doctor;

ὁ, ἡ καλοφαγής, mascul. & femin., τὸ καλοφαγές, neuter, does not occur in any author, but it is formed after the analogy of such words as εὐλογοφαγής (reasonable in appearance, plausible). So that καλοφαγής, ές, must mean beatiful in appearance, or beautiful to behold. The Latinized form Calophănes should be accented on the antepenult, ca lo'phanes, like Aristophănes, Theó-phănes, and the like. Such compounds come from

the short root of φαίνω, which is φăν. But if the author had κᾶλον in his mind, which is an old word for ξύλον, wood, then cālŏphănes must mean woody in appearce. But you can decide that.

Nota Bene; I expect to see you some of these days: I understand cold weather is not to your taste. As to Madame, I learn she is well.

ὁ σός

E. A. Sophocles

Our friends, the Osmanlis have been smashed up most abominably; the Greeks are in a state of commotion. But they have neither armies, nor fleets, nor heads.

Herbert Spencer

38 Queen's Gardens, W.

June 9/82

Dear Mr. Asa Gray,

Did I, or did I not, write in response to your kind invitation to visit you during my visit to the States? There has come upon me the suspicion that I did not write; and lest this should be so I write now.

Thank you very much. I have thus far, however, made it a policy not to commit myself to any visits; inasmuch as my health is such that I cannot bear any considerable amount of social excitement; and the

position of a guest, notwithstanding all pressures, invariably proves to be one in which the avoidance of social excitements is difficult, not to say impossible. I am, therefore, intending to live in nearly every case at hotels, where I am complete master of my own arrangements.

As at present intending, the only cases in which I may make exceptions will be in the case of those with whom I have long had intimate personal relations: namely, Prof Youmans & Mr. Fiske.

Nevertheless, I apprehend, hope to have the pleasure of seeing you & Mrs. Gray while I am in your neighbourhood.

I am

Sincerely yours

Herbert Spencer

Newport 25 Oct 1862

My Dear Professor Gray

I am obliged by the invitation contained in your note of yesterday and agree in the belief that since the dinner is so early in the day the excitement will not be too much for me

I was about to write expressing the hope that some arrangement could be made by which I should be enabled without too much fag to meet those I should most like to see and the little dinner you describe promises to serve the purpose exactly

We will arrange to arrive in Boston on Saturday morning in due time I say we, meaning my friend and myself - will you then permit extension of the invitation to him

Ever very sincerely yours

Herbert Spencer

Professor Asa Gray
Botanic Garden
Cambridge

TELEGRAPH STATION,
SUNDRIDGE.

COMBE BANK,
SEVENOAKS.

20 August 1881.

Dear Dr. Asa Gray, We are truly sorry not to have the pleasu
-re of seeing you on Sunday next ; but are sure that you must have
difficulty in fulfilling all the engagements with all your friends.

We also are looking with the greatest axnxiety and sympathy
at the daily bulletins of the health of your President.

Believe me yours very truly,

W Wodehouse

Boston, March 5/5

My dear Doctor.

As you surmise, my time and purse are almost exclusively occupied, so far as Natural History is concerned, with the pursuit after the recondite knowledge which lies hidden in those insufferably little pustules and stains and that living death which make up the little world of Fungi.

But I am unwilling to allow the opportunity to pass which you hold out to me to obtain what I may at some future time desire to study. I enclose therefore the sum you state and should be glad if you will send me in the specimens per express

Yours truly,

Thad. A. Sprague

J.

March 7. 1861.

My dear Doctor,

I am glad of a chance to be of use to you. I send you a copy of this morning's "Post" containing your notice of Silliman's Journal. I have also written a notice of your pamphlet on the Darwin theory which will be published in the Transcript. I thought that it would get a much larger circulation in this way and meet the eyes of more people interested in such matters. My acquaintance with editors is such as to enable me to get a moderately long article printed in several of our papers.

I like your pamphlet much. It pleases me more, because it appears to me that you have been able to throw off the bonds of early impressions, to emancipate yourself from the bonds of traditional theological creed, and break away into the pure air of untrammeled

our head. But this is just the sort of impossibility by which even Deity is, so to speak, circumscribed. By this I mean that Deity can only work with the existing means at his control. (I say this with all reverence and reverence and to endeavor to convey my idea) The changes which the world has witnessed are those of long time and slow divergences. I believe this to be at once the necessity as well as the policy of natural economy. This is, of course, mere speculation, and I should never pretend to even speak thus to persons who saw no farther than the pale of their ~~of~~ own church. But, as we sit thinking in this world, ideas and fancies will flow in on us which we cannot define, cannot describe, cannot verify — and yet which have the force of truths to our inner natures. Such are the imperfect and yet dimly defined ideas which we all get of God and of the workings of Creation. The theory of Darwin fairly explains much which was obscure to me before and points to what I have long dreamed of — that the whole diversity of nature springs from two single antagonistic forces which exhibit themselves reflectedly in such local points as the positive and negative in magnetism and the male and female in zoology. I have doubtless amused you with my heretical dreams, but when we touch such topics as original creation none of us can help a little latitudinarianism.

Yours very truly,
Chas. Jas. Sprague

thought and reason. When a devout believer like yourself has the boldness, the manliness, the common sense to view facts from the stand point of ratiocination and not merely through the lens of theology, we may have some hope of leavening the heavy, leaden, soggy mass of stupid bigotry, and inducing it to rise from its shallow pan of hereditary ignorance.

My knowledge of natural science does not enable me to weigh the theory of Darwin by the measure of fact. But I can and do grapple and hold to the ideas he advances. I am no theologian, scarce a Christian in any of technical meaning of the word. I believe in nothing, except that some vast power guides and pervades the Universe. His theory of Darwin appeals directly to my judgment as the most simple and logical one ever started to account for the facts in our possession. Running verbosely, as I do, the whole framework of main religion believing that it is utterly and entirely of man's fabrication, I have no hindrances, in the way of incompatibilities with the Bible, to a full acceptance of this new idea. And I fully believe that, carried out as it must be, it points to this fact. That the Universe today is merely the kaleidoscopic presentation of a changing creation that had no beginning and will have no end. Yesterday, today, and tomorrow have three new phases; and that these changes, inherent in creation, have been going on during the eternity before us and will go on through the eternity after us. I have no belief that there was ever any direct agency outside of creation working upon it in localised action. But I firmly believe that, if I can so express it, the Universe is the Deity, self-working, self-changing and making and changing through the elements which compose it. It is sophistry to say that God cannot make 2 x 2 five or a man eat his

C J Sprague

Boston, Jan. 29. '65

Dear Doctor,

Hunting among my papers some days ago, I came upon these, the work of my early efforts at botanical drawing. If they can ever be of any use to you, pray keep them, for I have relinquished my studies in that direction. I have no longer the time nor the opportunity to continue them. I have given considerable of my midnight oil of late (oil is now poetic for gas) to the more easily cultivated field of literature. I shall delve therein hereafter, and take Darwin, Spencer, Huxley, Tyndall &c for the companions of my lazy hours. As my copy comes along, I will send you samples.

Yours ever sincerely,

Chas. J. Sprague

Boston, Aug 16 1872

Dear Doctor.

I have just had an interview with Mr. Sargent who brings a letter from Mr. Hemenway. They both think, and I agree with them, that if they should put property in my hands, bearing a certain interest, circumstances might affect that property so that the interest might be reduced. They therefore propose to give me a sort of written promise to pay, each, $500. a year for five years, to be paid over to you, wherever you may be. This agreement commences September 1st. next. They wish me to say that they will pay it in quarterly or half yearly payments, as you may prefer.

So you see, my dear Sir, that you are started on the new work with the certainty, for five years, of $1000. a year. We presume that this period of time will be sufficient for the completion of the Magnum Opus.

I trust that your health will continue robust throughout, and that, by undivided attention to this final monument to your fame, you will complete it successfully.

I dont believe that "field-days" are just the things for you now; but I sincerely hope that you will be able to labor many years more in the quiet and comfort of your work-room.

Please let me know whether this meets your approbation, and how you would prefer the advances made by your friends.

With the warmest wishes for your success in the new work.

Very respectfully,

Chas. H. Sprague

Boston, Dec 27. 1873

Dear Doctor,

If you are pleased with the the x mas Diploma, its object is accomplished. I have so high an estimate of Asa Gray that I think that the Royal Society, London is honored in enrolling him among its members. That name will be known to science, not only over here in barbarian America, but wherever the flora of the world is studied, for centuries to come. Generations hence "A. Gray" will be the authority for the baptism of plants, which now are blossoming in untrodden wilds, but which then will grow around the suburbs of populous cities. Original editions of the Magnum Opus will be sought for by curious students, and be the grand authority of American botanical science. So mote it be!

I have looked at your sketch of Agassiz, and could not help being a little amused at the dexterity with which

you seized upon all points of his character which would bear eulogy. While I must acknowledge, with every one else, the great magnetic influence of his nature, the faculty he possessed of enlisting the interest of young students, of attaching them to him by a je ne sais quoi of manner and enthusiasm, I have never been able, myself, to get up much admiration for the man, nor a conception of his scientific position in the world so exalted as the popular estimate. I have heard of so many instances of his ungenerous treatment of brother scientists, his use of their labors to their aggrandisement & the name of Agassiz; I have been so unpleasantly impressed with his later efforts to popularise his reputation by a constant personal display of himself on all possible occasions, while his real, important study of nature was put aside; I have felt so strongly the rigid mind, so aptly pictured by Fiske, which refused to recognise patent truths which apparently conflicted with the theology of his surroundings; I have been so disgusted with the whole atmosphere of scientific pedagogy which enveloped him, while such men as Gray and Wyman were quietly, unostentatiously working away at their legitimate pursuits, conscientious and unambitious of an empty popular applause — that I have not been able to see the God-like man in the Cæsar I have found very human attributes belonging to him. But any organ of veneration is very small and I never could get up any hero-worship. When his works are collected, and the labors of the minds eliminated, I imagine that they will not possess the acumen nor the magnitude which ought to characterize a reputation such as he possessed. But I may be ignorant. I, of course, should not deem all this only privately to a personal friend. I should not dare publicly to criticize an Agassiz.

Yours very sincerely,

Ed. J. Wheeler

Boston, May 9, 1878

Dear Doctor,

Thank you for the very beautiful copy you have sent me of the new Flora! And accept my congratulations upon the successful completion of this volume. It is a sort of surety for the others. I hope that you will reap a pecuniary result worthy of the added reputation it will give to one already foremost in the ranks of botanical science.

I was about sending to you my subscription fee, but the very kind and friendly way in which you send the volume to me presents my giving the material help I was very ready to offer. All is you have my warmest and most cordial hopes that you will live to complete the

great work and that your present keen powers of perception and analysis may outlive the final presentation of the last cryptogamic order to the world; in "Gray's Synoptical Flora of North America"!

Yours very truly,

Chas. J. Sprague

Boston, Aug. 29. 78

Dear Dr.

I have read your lecture with much interest & profit. Of course each one want, to solve the problem in his own way. The prairie one is; it seems to me, that since the glacial period and the drying up of the great sea which has left its mark on the desert rocks, the forests have never re-established themselves because the area has never yet been in a condition to permit it – if, ever, there ever were forests there –. You allude to the matter at the close of your first section." It does not follow that they would re-occupy &c" Because under certain circumstances an area of country ages ago bore forests, there is no reason why under totally different circumstances of soil, accumulation, exposure &c. they should re-establish themselves, which they must do by seed. An acorn dropped on an ash heaps or a dense sod of grass roots does not germinate because it is an acorn. Myriads of seeds lie under our forest trees next, because, with rain, sun, soil & everything which a plant requires, in plenty, the exact circumstances are not what that seed requires. Beach plants dont grow in the marsh behind the beach. Now I can understand that there never have been forests in the extensive reaches of prairie & desert lands since the period of the last great atmospheric change because (– if there ever were –) the circumstances have never

Plants of more elastic habits might accommodate themselves to the new condition easily.

I suppose that there may be numerous theories put forth to account for these mysteries. I only put these thoughts down because they occurred to me as I read your lecture. I wish you had more time for similar work, for the elucidation of theory. But one cant do everything.

Yours very truly,
Jno Jeffries

The history of our Kalmia is apposite here. You and I thought it, at first, introduced, till a broader view led to the belief that it was the scanty remnant of what once was a wide-spread tenant. It will grow cultivated anywhere; but Nature does not cultivate it in New England to any extent. So with the Sequoia. It wont travel. It likes home. It dont find home anywhere else. Its relatives have been all killed and it shrinks from leaving the only place it is comfortable in. I suppose if it could be induced to visit Asian mountains it might colonize there. But I dont believe it would ever get a girth of 100 feet in Europe, away from its Pacific breezes.

yet secured, under which, they primevally grew.

Then as to the richness of Japan & the poverty of the Sierra. Yeddo is in about the same latitude as Richmond, San Francisco. The glacial discharge from the Sierra would be south-westerly into a profound ocean. The sweep of its destruction would be into a watery grave. In Japan the southerly movement of the glacier would be towards what is today an Archipelago, but which in remote times might have been a continent or, at least, an aggregation of large islands capable of receiving, retaining and returning the Flora crowded south by the advancing glacier. In the same way the Eastern U.S. have an archipelago to which their Flora might have retreated in a similar manner. The results would be that the Californian flora might have perished in the waters of the Pacific, save such plants as for some reason retained their vitality, while the U.S. (east) and Japanese flora might have maintained itself (themselves) on southern shores, returning when the ice withdrew, to their old habitats. We must make great allowances for upheaval and subsidences during these long periods of time. Another thing - the Californian areas of vegetation are, I think, much more elevated than either the Atlantic & Japanese areas. The deciduous trees which returned to Japan & here, found, at once, an elevation which was favorable to their life. But they had to climb up the immense plateaux of the Sierra to reach a place in which they had never lived before - that is, an elevation to which they were (I think there are evidences of great upheaval out there.) not accustomed. But the conifers which did survive the ice reestablished themselves in those altitudes naturally. I find that 7800 feet is given as the hight of the highest Japan mountain, not much more than Mt. Washington. and I think that the general country from which the Sierra rises is several thousands of feet high. Now it may be that the Sequoia requires a certain elevation for its existence. The Asian mountains are high enough to be sure, but south of them comes the great Indian sea, and this, like the Pacific, might have been an agent in their destruction. We might even suppose, in their case, that when crowded down into warmer climes they perished from lack of their wonted condition. It is not necessary to suppose that the refrigeration extended proportionally south, because today, the Alpine glaciers do not refrigerate the atmosphere so much as to prevent rich verdure up to their very bases. But the Sequoias, being shoved into such a climate might die, as our Buttonwoods have here for no cause that we can see, while other

Boston, Sept. 1, 1878.

Dear Doctor,

I enclose my check for $500. the last instalment of the $5000. contributed by Messrs. Hemenway & Sargent.

I must avail myself of the occasion to congratulate you on the results of the past five years, on your continued good health, active mind and body, and on the prospects of the continuance of the "Flora" without hindrance from the cessation of this special money assistance. I hope to see the full completion of the work from your hand.

And I congratulate myself that I was permitted to be an humble agent in the initiation of this great work, a mouse which chanced to be able to free a lion from his shackles.

Yours very cordially,

Chas. S. Sprague

L. J. Sperry

Boston, March 2. 1880

Dear Doctor,

I am so glad that you have taken my criticism so kindly, in the same spirit as it was written.

I can readily answer your question. I do not think it necessary to take any position. What I believe is the outgrowth of long, serious, attentive consideration of the subject; is endogenous. I don't think any man can argue himself into any belief or disbelief. What makes up the convictions of his mind and nature is the gradual and strengthening growth of ideas which observation and reflection engender.

My creed is perhaps disbelief rather than belief. I have arrived at the firm conviction that everything of a theological nature, as exemplified in the various religious beliefs of mankind, is the work of man, the results of his fear, his hope, his conceit, his ignorance in a past time, his superstition in the present, the lingering, inherited priest-propagated

fetish-worships of antecedent mind-states. Then I dismiss in one crowd all of the thousand sects, Christian, Pagan, Jewish, Mahommedan which have fought each other through ages, no two agreeing. They are all, to me, the creations of man's self-magnification in the scale of being.

Now to answer, more directly, your question. Here comes in my agnosticism. I dont pretend to fathom in the faintest degree what the Great First Cause is or may be. That he is a "Father" (I say he because language must be used somehow) in my sense of Father, does not appear to me to harmonize with any human idea of what a Father is. But in the absence of any finite power which can conceive what the Power can be, I have arrived at this impression, that all mind, so called, is the dynamics of matter; that our emotions, desires, aspirations &c-&c, are the same to us as the flame to the candle, they endure while the action of life is going on, they cease when that action ceases. The laws of Nature are utterly ruthless in their treatment of Man's noblest attributes. (We say noblest, because we are men) They trample cruelly on the nicest, most delicate and (what we think elevated) impulses and sensations. Therefore am I compelled to the impression that whatever the Power may be, it is totally different from anything of impulse and sensation men experience.

With these impressions I simply feel that it is impossible for me to have the faintest conception of what the Power is, and therefore I have but one Creed, unending, I am perfectly willing to acknowledge, from my nature rather than my ratiocination: — "Do all the good you can to those about you and you will be happier for it! Leave, in your utter ignorance, the rest to whatever will be, in spite of your conjectures, for which you have no particle of foundation."

If I should write a folio I could not more express my very short Credo.

Yours very sincerely,

Chas J. Sprague

Boston, Nov. 2. 1882

Dear Doctor,

I knew nothing of your misfortune and am most heartily glad that the announcement of it comes to me by your own hand, which tells me that you are really "doing nicely." It is no trifling matter for a man of your age, and I sincerely hope that you will entirely recover the full use of your right hand - of so much importance to the world without, as well as to your own personal comfort. You have a remarkable vitality and buoyancy, both mental and physical, and I expect that you will become an octogenarian with the same freshness and cheerfulness and mental grip which you now exhibit in spite of a broken shoulder blade.

Ever yours sincerely,

Chas. J. Sprague

Boston, Sept. 8. 1884.

Dear Dr.

I have received the elegant volume you have sent me, with its condensed wealth of botanical lore, and hasten to express my hearty thanks for the continued expression of your cherished friendship. It is with no small pride that I put upon my library shelves and leave for my children and grand-children to see, the inscriptions these volumes bear. Years hence, when we are both gone, they will point to these pages and say: "See! these were presentation copies to grandpa from the celebrated Dr. Gray, and they were friends together." Thus shall I too have a posthumous fame, which shall be a reflected light from your brilliancy.

I hope that now you will be

able to give your attention to minor works, which while they will be equally useful to the community, will be also of profit to yourself, such as a new Manual.

I am off for the Rangeley Lakes with my son, this week, and shall be back again in Boston in early October.

Yours ever sincerely,

Chas. J. Sprague

which drew from him warm thanks with a !

Towards the last he became overborne with work, disgusted with labors sought to be forced upon him and, in his last letter to me, he evidently wished me to understand that he did not want me to send him anything more. This was one reason why I concluded to give my collection to the society and withdraw from the study; as it was futile for me to employ collectors when what they collected must remain in my hands. If the Dr. could not elaborate them, I should be forced to send to Europe and that I would not do. In fact, I was getting weary of the microscopic work and my eyes were feeling the strain.

As to any public notice, you must excuse me. I know nothing whatever of the Dr's life and do not feel at all competent to undertake it. There are many who were brought into his sphere of knowledge who ought be able to do this acceptably.

Yours very sincerely,

Chas. J. Sprague

Boston, March 24. /86

Dear Dr.

I am sorry that you caught a cold at Amherst. I am almost willing to say that I never attended a wedding or a funeral that I did not catch cold. My winter sufferings began this year, with a violent bronchitis caught at a wedding in a church.

The fact is that I am getting to feel the instability of equilibrium which is characteristic of increasing years. I am a much older man than you are, so far as both mental and physical vigor and ability go. Your home has a far more pungent meaning to me than to you. You may have noticed a desire for retreat, a dislike of exertion a shunning of society which I may have exhibited in the past few years. The fact is that my nervous nature has been unstrung and I have lost that vivacious buoyancy which characterized my life in earlier years. I have withdrawn from nearly all participation in the active pursuits which once possessed for me a vital charm, and feel more content at home with my children and grandchildren

than in society abroad. Hence this personal explanation of a certain reluctance to do anything or go anywhere outside of my customary daily round.

Mr. Willey's letter is a sad one. I never knew Dr. Tuckerman and, except the purely scientific correspondence of the last few years, never had any communication with him. I know that he was peculiar, even eccentric, from what I have heard. The reason of the lack of interest in his work on the part of Europeans was doubtless that he differed essentially from them in his ideas. They are all species makers. He had a far wider grasp of his subject, I think. He became alienated from the more celebrated lichenologists. Of course I could not pretend to give the underlying causes. But I know, from what he wrote me, that he was bitter against Nylander. I am inclined to believe that the genial, fraternal, sympathetic feelings which have characterized your acquaintance with so many leading scientists of the world, were not the experience of Dr. Tuckerman. When I asked him if he did not want duplicates of new things to send to his correspondents, he answered that he made no European exchanges now. Then again, the recondite character of the specialty he studied kept him aloof from all except lichenologists, and there are but few of those now who are his peers, and with those few he differed. I imagine that the younger lichenists, who are all followers of the grand Mogul, Nylander, would scarcely feel the impulse to admire or appreciate an American of 66 who would be old foggy in his ideas, to them. My experience has been that men of science, as a general rule, are extremely sensitive and apt to be touched and offended at what they consider encroachment upon their domain; from which remark I except one Asa Gray, who has too large a nature to be influenced by trifles. But I can understand that a rather precise and punctilious person like Dr. Tuckerman might not have the magnetism to win and retain that certain affection which you have enjoyed, even though the medium of its expression was a scientific correspondence. I know but little of lichenology, merely a smattering picked up in a few years' observation. But I learned to believe that Dr. T. had far the wider, intellectual grasp of the subject than the present Europeans exhibit. It is to be regretted that he did not live to finish his Synopsis, and it is to be hoped that Mr. Willey may be permitted to publish his manuscripts. His herbarium must possess very many unpublished species, which would have been given to the world in Part 2d. He was not inclined to publish new species if they were not something of peculiar interest. He could work but a few hours a day and did not turn out work as many do. But he was accurate when he did publish. He was very kind to me always and I was always particularly pleased to send him, as I often did, material

Boston, Jan. 9./87

Dear Dr. Gray,

Ever kind, ever genial, ever thoughtful!

Thank you for your pleasant letter of introduction! I shall certainly avail myself of them whenever I go in the localities.

I should scarcely be able to go to Cambridge again before I leave. But I send my cordial leave-taking with the hope that I may find you, on my return, as bright and cheery and hearty as I left you at Brookline.

With my warm regards for Mrs. Gray.

Yours ever sincerely,

Chas. J. Sprague

N Boston, Oct. 17./88

Dear Dr.

The vicinity of Boston is a field so thoroughly explored that there is little to add to the Manual and little to correct. Still I have run across a few things during the 20 years past. I have generally notified Dr. Gray and I presume that he made note of them. I send a short list.

As to the desired improvements at Cambridge, I am altogether too sympathetic with you in the matter of asking for money. Improvements would come very slowly were they dependent on my alms-asking for even high purposes. As to myself, I have a fond remembrance of many hours of peaceful pleasure passed in that room, where the Dr. used to work and where he received me so genially forty years ago (!) - I, an outsider.

I will willingly contribute $25. to your necessities, if that will help you on, out of sheer retrospective affection.

Yours sincerely,

Chas. S. Sprague

Boston, Jan. 14. 89

Dear Sir,

Only one more question to bother you.

Shall you follow Hooker & Bentham in the sequence of Genera in the New Manual or continue the old sequence of Dr. Gray?

Yours truly,

Chas. J. Sprague

Hoboken, June 26 '89

Dear Dr.

I hurry off the enclosed, just found by Miss Ellen Lincoln in this town, because it totally confounds me, and I want you to see it as fresh as possible.

Thanks for your kind offer to revise our list! We may be impelled to take you at your word.

I have made a nice specimen of the Panicum intermedium for your herb.

I have also a grass to send by a boy.

Yours sincerely,

Chas. Jas. Shepard

Wingham,

Aug. 27./89

Dear Dr.

Did you ever see such a red Monotropa Hypopithys?

How is the Manual getting along? Can you fix a time for its appearance?

Yours truly

Chas. J. Sprague

380 Marlboro St.
Feb. 2. 1890

Dear Dr.

Tout vient à celui qui sait attendre. Thank you very much for the New Manual which comes à propos! I can now adopt your names for our list of Wingham plants which will, I suppose, be printed this spring.

I have not waited to give it a close examination. In looking it over casually I notice one good change, - the printing of the introduced species in close type. It catches the eye.

I suppose that you will bring down upon your head the

anathemas of the Brittonian and Kreenian and even the Bargentian race for adhering to Dr. Gray's convictions and refusing to recognize Gastalia, Hirekoria rc. rc. However some of us old fogies will be glad to be permitted to use familiar names for familiar things. I was taken aback a few days ago to find that Acer saccharinum was not the Sugar Maple at all, and I began to think that I was too old to recommence the study of New England Plants in a new language.

The Manual looks well and it will be the vade mecum of the New England student, at least for many years. It has my hearty welcome.

Yours very sincerely,

Chas. Sprague

Feb. 22. 1890.

Dear Dr.

I wish to buy some copies of the New Manual. Can you direct me where to go here in Boston?

I have enjoyed the study of it and have already got our Wingham Bird named in accordance with it. While the comparatively few profound scientists are studying your far more extensive original works in their libraries, hundreds of students, with this book in hand, will have your name upon their lips for years to come.

I see that my Examine figures in the Appendix.

Yours very truly

Chas. Jas. Pennie

Dear Dr.

I have just received the March No. of the Torrey Bulletin and I am impelled to express to you my disgust at the coarse and vulgar attack it contains upon you and your coadjutors as Editors of the New Manual. It would seem to be instigated by personal animosity; for it seems hardly credible that a mere difference of scientific convictions should permit any one to thus virulently arraign a brother scientist.

I know that scientists sometimes forget their dignity and descend to personal vituperation. We have had instances of this here as well as abroad. But I am astonished that the Managers of a Scientific Journal should

Sprague

allow such an insulting onslaught to be published in its pages. Honest criticism of scientific opinions is everywhere allowable. But assail the scope, the bulk and the type of a work as "defects," and then ask why it was not made more defective by adding other matter, already elaborated by another author in a publish-ed volume, itself almost as large as the one in question, shows an animus which is animosity.

The particular grief seems to be that Pennsylvania was not specially mentioned as the habitat of some plants; although in several of the cases mentioned, the general statement that they occur "and S." covers that state.

The seven questions propounded at the close refer, with one exception, to scientific conclusions, about which men may differ, but which are certainly no ground for such a scurrilous attack. If it be personality, the Editors of the Bulletin have derogated from the position they should occupy and have shown how small distinguished men can be, apart from their special acquirements.

Yours sincerely,

Chas. J. Sprague

380 Marlborough St.

March 18./90

April 9./90

Dear Dr.

A friend of mine is going to the Rhine & Switzerland pretty soon and he wants to botanize as he goes. Can you give me the names of any Manuals of those regions, which he could obtain over there? No matter what language. He knows them all, and is something of a botanist in an amateur way.

I shall be obliged for any suggestions you may make.

Yours sincerely,

Chas. J. Sprague

Hitchcock, July 18/90

Dear Dr.

There is one doubt which I wish to solve before I include the Sisyrinchium angustifolium in the Hitchcock list.

We have found a few scant specimens of this, if the marked difference in the length of the bracts, - one being twice the length of the other - and the single spathe are sufficient to define it. But so far as seeds go, we cannot find the difference laid down in the Manual. The common form we find here is a tall, coarse one growing in grassy meadow lands with branching stems, numerous spathes with equal bracts. Whether this rare smaller form is a scanty specimen of this other,

notwithstanding the marked difference in the bracts, in our question. Mr. Sayon has sent me a few specimens of it with fruit, and he queries just as we do.

I send you specimens of both and shall be obliged if you will solve our doubts.

Yours sincerely,

Chas. J. Sprague

Wingham,
July 26./90

Dear Dr.

I send you a Penstemon which I found growing with Rudbeckias in a redeemed meadow here.

Is it a forerunner of the various western immigrants which are creeping in? Or is it merely some chance stranger?

If it be worth keeping, will you kindly return it?

Yours sincerely,
Chas. J. Sprague

Sun of Isaac Spencer

Wellesley Hills,
January 26th, 1890.

My dear Madam:

My Father asked me to say to you that your note to him was received in due time, but that, on account of his absence from home, it has not been acknowledged as promptly as it should have been.

He is sorry to say that he has no sheet of the plates you ask about, and has only a few impressions of some of them made on thin paper, while the work was in progress

and no full suit which he thinks) could be made available.

He desired me to thank you for your invitation.

Respectfully yours

Isaac Sprague, Jr.

Mrs. Jane E. Gray

Botanic Garden Cambridge

Mass.

Autograph

Welburn - York.
11. Nov.

Dear Sir

I furnished to Dr. Gray (for Mr. Sullivant) a small set of L. American Mosses for which he paid me £3 - 10 - 0. I also gave him a box full of remainder duplicates. I have nothing more Sullivant-wd care to have, unless he have a purchaser for the only remaining small set of my mosses, price £3; or unless he choose to put down his name for a set of the Hepaticæ (the whole of which are still undistributed.)

Thanking you for taking the trouble to write me about it I remain

Yours ever truly
Rich. Spruce.

Prof. Oliver.

Please put the enclosed in Mr. Baker's hands, and ask him, when he is next overhauling the Hymenophyllæ, to put down the names of those of my gathering — according to the latest recension — and return the document to me.

Welburn near Castle-Howard,
To. Yorkshire. May 25, 1847.
Messrs. Wiley and Putnam

Gentlemen,

Herewith I entrust to your care a packet of plants for Professor Asa Gray of Cambridge, Massachusetts, according to his request, and shall feel obliged by your forwarding it at your convenience.

I remain.

Gentlemen
Your obdt. servt.
(Signed) Richd. Spruce.

R Spruce

Welburn near Castle-Howard,
Yorkshire. May 25, 1847.

My dear Sir,

Herewith you have a packet of mosses which you will be so kind as to remit to your friend Mr. Sullivant. I send it, as requested, through your London publisher, & shall be glad to receive a return packet through the same channel.

I have not been unmindful of your wish to possess some of my Pyrenean flowers, but as some of the choicest of them have been already distributed among my subscribers and as any American Phanerogamia would be thrown away upon me, I have requested my friend and fellow-traveller, Dr. Southby, (Bulford, Amesbury, Wiltshire) to make up for you a packet of Pyrenean plants, which he has accordingly done and will send it to the care of Wiley & Putnam to meet my own for Sullivant. He paid more attention to the Phanerogamia than I myself did & collected several species which I did not. In his earlier years he herborised a little in Canada & I know that any American plants will interest him very much. I believe he possesses a considerable collection of Australian plants, some of which he would I dare say be willing to Exchange with you for such as you can furnish.

Believe me, Dear Sir,
Yours very respectfully
Rich.d Spruce.

Prof.r A. Gray

Prof. Asa Gray

Welburn
York.
20. Oct. 1869.

My dear Sir

I have just learnt that the Mosses were sent from Hurtpierpoint to your addres yesterday, & I suppose by this time you may have received them. I pay for packing & dispatching them 5/- which added to the price of the Set-maker £3 . 10 . 0 that you will owe me on Mr. Sullivant's account. You can send me that sum either by crossed Cheque or by P.O. Order payable at Malton.

If you have anybody near you who can be trusted to handle mosses, I have little doubt the parcels may be much lessened in bulk.

I am, Dear Sir,
Yours very faithfy
Rich.d Spruce.

Professor Asa Gray.

Over

Could you give me any information of the whereabouts of any of the following gentlemen:

Mr. Hassaurek, who was minister at Quito in 1863. He promised me a copy of his book on Ecuador, but probably he knows not where to send it, even if so disposed.

Dr. C. F. Winslow, U. S. Consul at Payta at the same epoch. I consulted him, as a physician, and his advice was "Laugh some every day," for which I paid him ten dollars. It was cheap at the price.

Mr. Camillus Farrand, a clever photographer whom I met in the Andes. He was connected with Mr. H. T. Anthony of Broadway, New York.

CHAS. W. SPURR, PRES'T. WM. E. UPTEGROVE, VICE-PRES'T. JEROME P. UPTEGROVE, SEC'Y & TREAS.

Chas. W. Spurr Company,

Sole Manufacturers of

Spurr's Patent Veneers, Marqueteries and Wood Carvings,

35 Bedford Street,

PRINCIPAL OFFICE:
465-467 East 10th St., New York.
Factory, Sandwich, Mass.

Boston, March 19th, 188 8.

Prof. S. Watson--
Botanic Garden, Cambridge, Mass.

Dear Sir:-

I am very sorry not to have seen you when you called, and will endeavor to give you the information you desire.

Of course, you understand that the specimens we have were got from the celebrated "Jesup Collection of N. Am. Forestry", embracing [nearly 250] varieties. These woods have been cut, prepared and mounted on single cards, numbered, Latin and common name and authority on each, and correspond exactly with the book published by the U. S. under the direction of Prof. Chas. S. Sargent.

201

It has been my intention to place on each card the name of the College or University purchasing, and the name of the Director or President at the time, and to enclose the specimens in a nice Cabinet of three drawers, each compartment containing three sections of the same wood in its transverse, radial and tangential cuttings.

As I understand it, more than $150,000 has been expended in collecting, preparing, mounting and perfecting this collection, and I make you the offer of one complete set in the Cabinet at $550.; the actual cost of preparing and mounting being more than $550., to say nothing of the $65,000, cost of collecting, with freight and three years labor of professors and mechanics since, amounting to more than $150,000, as has been said.

Trusting that this information is what you desire, and that you will be able to purchase one of these sets,

I remain most respectfully yours,

Chas. W. Spurr
Pr. M.A.W.

P. S.

These can be examined at any time, if you so desire and also the book accompanying the set.

= #1.75 per wood.

1–6, 10. 14. 16–19, 31, 33–34. 37. 39. 47. 52. 54. 56. 57.
60. 61. 63–64. 77. 82. 85. 86. 89. 91. 93. 94. 101–103. 105.
108. 117–119. 123. 129. 137. 142. 145. 146. 151–153. 156.
165. 166. 169. 175. 177–179. 183. 184. 186–189. 191–193.
196. 199. 201. 206–208. 210. 211. 213. 215. 217. 218.
222. 224. 226. 227. 230. 232. 236–240. 242.
244. 246. 248–250. 252–254. 258. 260. 262.
264–268. 270. 272–274. 276–278. 280–285.
287–292. 294–299. 301. 303. 306. 308. 309.
311–313. 316. 319. 321. 326–333. 337–340. 343.
345. 346. 348. 349. 353–358. 361. 363. 364. 366.
368. 369. 371–374. 376. 378. 380–385. 387–392.
394–396. 398–401.

= 201

SN 3

Memo of Sections made up from Short-Guola for Cabinet #2

No.	Trans.	Rad.	Long.	No.	Trans.	Rad	Long.
2	1	2	3	210	1	2	3
3	1	2	3	211			1
4	1	2	3	214	1	2	3
6	1			249	1		3
8	1	2	3	260		2	3
16		2	3	264	1		3
18		2	3	265		2	3
31		2	3	266		2	3
34		2	3	279	1		
52		2	3	287		2	
57		2	3	289	1	2	
61	1	2	3	290	1	2	3
63		2	3	301		2	3
77	1	2	3	312		2	3
91	1			319	1	2	3
94	1	2		321			3
105	1			323		2	3
119	1	2	3	345		2	3
141			3	354		2	3
145	1	2		361	1	2	3
148			3	373		2	3
151		2	3	374	1	2	3
152	1	2	3	380	1	2	
165		2	3	383	1	2	3
175		2		385	1	2	3
177		2	3	389	1	2	
178	1	2		395	1		
179	1	2	3	413	1	2	3
183		2	3	414	1	2	3
187	1	2					

CHAS. W. SPURR, Pres't. WM. E. UPTEGROVE, Vice-Pres't. JEROME P. UPTEGROVE, Sec'y & Treas.

Chas. W. Spurr Company,

Sole Manufacturers of

Spurr's Patent Veneers, Marqueteries and Wood Carvings,

35 Bedford Street,

Principal Office:
465-467 East 10th St., New York.
Factory, Sandwich, Mass.

Boston, March 23rd, 1886.

Prof. S. Watson--
Botanic Garden, Cambridge, Mass.

Dear Sir:-

We send you herewith the List of Specimens from the Jesup Collection, and trust it will satisfactorily answer your needs.

When you have no further use for it, please return to us.

Very truly yours,

Chas. W. Spurr
Per M&W.

CHAS. W. SPURR, PRES'T. WM. E. UPTEGROVE, VICE-PRES'T. JEROME P. UPTEGROVE, SEC'Y & TREAS.

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35 Bedford Street,

PRINCIPAL OFFICE:
465-467 East 10th St., New York.
Factory, Sandwich, Mass.

Boston, March 29th, 1888.

Prof. Sereno Watson--
Botanic Gardens, Cambridge, Mass.

Dear Sir:-

Your favor of 24th March duly to hand. We sent you memorandum of 201 varieties and as there were three specimens to each variety it made over 600 specimens.

I have another memorandum, which was neglected in forwarding you the list, to which are added 59 varieties(additional to what I have sent you), and 126 specimens of these 59 varieties.

It makes a great difference whether you are figuring on 201 specimens, or whether you are figuring on 600 specimens, and 201 varieties.

If you wish the 201 varieties of 600 specimens, without the case and without printing, packed ready for shipment wherever you may direct, you can have them for $250. If you wish the 59 varieties additional, without the case and without printing, you may have them for $325.

In regard to the great expense which has attended the getting up of the Jesup Collection of Woods would say, that there has been paid out over $8,000 in cash at this end, besides the utilizing of my machinery and an immense amount of time, experiments and experience and for which there has been no return of more than $500. to $600. We think we come in for our proportion of the cost of preparing this collection.

Very truly yours,

Chas. W. Spurr

Dictated.

Direction des botanischen Gartens u. botanischen Museums der k. k. Universität
WIEN, III. RENNWEG 14.

Vienna 14/4 86

Dear Sir,

I am charged by the director of our museum, professor Kerner, to reply to your lettre and to confirm the receipt of the most interesting collection of American plants.

The next four centuries of the Flora exs. Austro-Hungarica shall be publicished still in this month and you will receive it as soon as possible.

Believe me yours truly

Dr Otto Stapf

Assist. on the bot. mus.

to Wats.). Texas: near El Paso (Varey, 841.)

To Ephedra pedunculata belongs probably also the E. antisyphilitica in Torrey, Bot. Bound. Serv. p. 207.
from Ranconado Pass in Coahuila (Thurber) and between Mapami a. Guajaquilla (Durang) and the plant coll. by Gregg (? 882 Wright);

What is E. sp., coll. near frontera and Ojo de Vaca in Chihuahua (Thurber)?
Where lies "Ranconado", "Guajaquilla" and where collected Gregg the" above mentionned plant?
The sign (!) means, that I have seen myself the plant.
The red ? means, that a new examination of the plant is very desired.

El Paso & Frontera are in [illegible]
northwestern corner of [illegible]

Proc. Amer. Academy are [illegible]
continuous series.

near Tucson (Parish, 1884, n° 142!, Pringle, 1884!) – New Mexico: Near Frontera (Wright p. 1884, acc. to Wats.) and El Paso (Bigelow, acc. to Wats.) – Colorado: ~~To~~ Near Mesa Verde and in the Animas Valley (Brandegee).

4. Ephedra antisyphilitica Berlandier

in C. A. Meyer, Versuch e. Monogr. d. Gatt. Ephedra (1846) p. 101. – Endlicher, Synops. Conif. p. 263. (1847) – Carrière, Traité gén. d. Conif. p. 560 (1855), éd. II. p. 777. (1867) – Parlatore Gnetac. in DeCand. Prodr. XVI. II. p. 357. (1868) – Watson Contrib. to Amer. Bot. in Proceed. of Americ. Acad. of A. a. Sc. N. Ser. VI. (1879) p. 299. in regard to the local. in W. Texas (n° 320, 1590, Berlandier) – Contrib. to Amer. Bot. in Proceed. of Amer. Acad. of A. a. Sc. N. Ser. ~~X~~. (1883). p. 157.

To exclud: E. antisyphilitica in Watson Contrib. to Am. Bot. in Proc. of A. A. of A. a. Sc. N. Ser. VI. p. 299. in regard to the local. in North. Mexico (= E. pedunculata) and in Watson Bot. (in King, Report, p. 328. (= E. Nevadensis)

Steppe of Western Texas: Between Laredo and Bexar (Berlandier, n° 1590, 1828!) – Near Sutherlandsprings (Palmer, 1880, n° 1292!) Uvalde Co. (Reverchon, 1428) Near Guadalupe, near New Braunfels (273 Lindheimer) Near S. Antonio (n. 1862 Wright, 1852)

Tas it belongs very probably the E. antisyphilitica in Torrey, Bot. of the Bound. a. S. a. M. Bound. Surv. p. 207. in regard to the local. "between the Nueces River a. the Rio Grande" and "from Frontera to Eagle Pass" and also E. antisyphilitica in Parlatore in reg. to the local. "Llano Estacado" and "Peco" (cf. Torrey in Whipple, Rep. 84 and Pope's Rep. p. 19.)

5. Ephedra pedunculata Engelmann

in Watson Contrib. to Amer. Bot. in Proceed. of the Amer. Acad. of A. a. Sc. N. Ser. X. (1883) p. 157. – Reverchon, Botan. Excurs. in Texas in Gartenflora, XXXV. p. 640. (1886.

Syn.: E. antisyphilitica Watson, Contrib. to Amer. Bot. in Proceed. of the Amer. Acad. of A. a. Sc. N. Ser. VI. 1879. p. 299. in regard to the local. in North. Mexico; Bot. of Calif. ii. 169.

Steppe of ~~Northwestern~~ the Northeastern Mexico and the adjoint parts of Texas and New-Mexico.

Mexico: San Luis Potosi, near the town of S. L. P. (Schaffner, 1879, n° 279!) – Coahuila: near Saltillo (Palmer, Mexic. fl. 1880, n° 1289!), near Parras (Palmer, Mexic. fl. 1880, n° 1290, acc. to Wats.) – Chihuahua, At the Bachimba Canyon, west of the town of Chihuahua, in Shrubs climbing to 6 feet h. (Palmer, Pringle, 1885, n° 134!) – Texas: near St. Valley of Nueces, Frio (673 Wright, 1852), near the Nueces (147 Reverchon) Uvalde (Palmer, fl. of S. W. Texas, 1880, n° 1291!) – New Mexico: In the valley of the Rio Grande, below Doñana (Mexican Bound. Surv., n° 1388!) Nueces bottoms, n. 225 Lindh.

6. Ephedra Nevadensis Watson

Contrib. to Amer. Bot. in Proceed. of the Amer. Acad. of A. a. Sc. N. Ser. VI. (1879) p. 298. – Botany ~~of Cal.~~ Surv. of Calif. II. (1880) p. 108. – Contrib. to Amer. Bot. in Proceed. of the Amer. Acad. of A. a. Sc. N. Ser. X. p. (1883) p. 157. – Coulter, Manual of the Bot. of the Rocky Mount. Reg. p. 428. (1885).

Syn.: E. antisyphilitica Watson, Bot. in King, Rep. of the Geol. Explor. of the 40th. Par. V. 1871. p. 328. a. p. XXXIII. pl. XXXIX. f. 1–16.

Steps and deserts of the Western States from the Mexican Boundary to Northern Nevada and Utah:

Kessen County (Mrs. Annis)

California: Rock springs, east of the Mojave Desert (Palmer, flor. of Calif. 1876, n° 52!) – Cushenberry Springs in the Mojave Desert (Parish, Plants of S. Calif. 1882, n° 1369!) (probably also: Fort Tejon, w. n. w. of Mojave Desert (Xantus, n° 112) and Mountains near Santa Inez (Brewer, n° 347) Cf. Watson a. a. O.) – Arizona: near Fort Mohave at the Rio Colorado (Cooper) – Nevada: Esmeralda Co. (where?) (Shockley 1881!) – Utah: N. Milford, South of Sevier-lake, 5000' (M. Jones, 1880, fl. of Utah, 1402!)

To it belongs also acc. t. Watson, 1879 l.c. the plant coll. by Gregg in North-Mexico. The local. is unknown to me.

Hither belongs probably also:

1) E. antisyphilitica from Zuni and Yampai River (cf. Sitgreaves, Report of a. Exped. (1853) p. 174)

2) E. antisyphilitica from Wyoming Terr. (cf. Porter, Catal. of pl. in U. S. Geol. Surv. of Wyom. a. port of contig. terr. 1871. p. 482.)

3) E. antisyphilitica from Arizona a. New Mexico, at least partially (cf. Rothrock, Reports l.c. – 1878. p. 50.

4) E. antisyphilitica from La Plata Valley (cf. Brandegee fl. of South west Color. – p. 243.)

7. Ephedra aspera Engelmann

in Watson Contrib. to Amer. Bot. in Proceed. of the Amer. Acad. of A. a. Sc. N. Ser. X (1883.) p. 157.

Steps of the Northeastern Mexico and the contig. port. of Texas: ~~Coahuila~~ In the Sierra Madre, south of Saltillo (Palmer Mexic. fl. 188, 1880!) – Chihuahua: In the Sierra Santa Eulalia near the town of Chihuahua (Pringle, Plants. Mex. 1885, n° 38!), near Carretamillo (where?) (Gregg, n° 53, acc.

1. Ephedra Californica Watson

Contribut. to Amer. Botany in Proceed. of the Amer. Acad. of Arts a. Sc. ~~N. S.~~ Vol. ~~XIV~~ XIV (1879). p. 300. — Watson Bot. ~~in Geol.~~ ~~Surv. of~~ Calif. II. (1880). p 109.

~~Steppes and deserts of~~ Southern California: At San Diego and in the Jamuel Valley near San Diego (Palmer, 1875, nº 365.!; Jones, fl. of Cal. (1882) nº 3076.!) — Indian Station (Vasey, 1880, nº 622.!)

/: NB. I do not find „Indian Station" in the map; where lies it? I suppose, in the depression territory of the Colorado desert:/ ~~In southern part of San Diego County~~ near the lower Colorado.

2. Ephedra Torreyana Watson

Contrib. to Americ. Bot. in Proceed. of the Amer. Acad. of Arts a. Sc. ~~N. S.~~ Vol. ~~XIV~~ XIV (1879) p. 299; Bot. Calif. ii. 109.

Syn.: E. antisyphilitica (f. monstrosa?), Torrey, Bot. of the Bound. U. S. a. Mex. Bound. Surv. (1859) p. 208. — E. trifurca Torrey, Botan. Observ. in South Utah in Americ. Nat. IX. (1875) p. 351.

Steppes of the Western States from the Mexican Boundary to Utah: New Mexico: Near frontera at the Rio Grande (Wright, nº 1883.!) and near El Paso (Bigelow, acc. to Wats.!); near Santa Fé (Rothrock, nº 80, acc. to Wats.) Utah: Salt desert in the upper valley of the Virgin River (Torrey, 1874, nº 250 acc. to Wats.)

3. Ephedra trifurca Torrey

in Emory, Not. of a Milit. Recomn. fr. f. Leavenworth i. Miss. to San Diego in Calif (1848). p. 152. — Watson Bot. in King, Rep. of the Geol. Explor. of the fort. Par. t. V. (1871) p. 328. — Brandegee, The flor. of Southw. Col. in Bull. of Geol. a. Geogr. Surv. of the Territ. II. N. 3. (1876) p. 243. — Rothrock, Rep. upon the Bot. Coll. in Wheeler, Rep. U. S. Geogr. Surv. west of the One hundr. Merid. v. VI. (1878) p. 264. — Watson Contrib. to Amer. Bot. in Proceed. of the Amer. Acad. of Arts a. Sc. ~~New Ser. XIV~~ XIV. (1879) p. 299. — Botany ~~in Geol. Surv.~~ of Calif. II. (1880) p 109. Coulter, Man. of the Bot. of the Rock. Mount. Reg. (1885) p. 428.

Syn.: E. trifaria Parl. Gnetac. in DeCand. Prodr. XVI. II. p. 359. (1868).

To exclud: E. trifurca Torrey, Bot. observ. in South Utah in Amer. Natur. IX. (1875) p. 351 = E. Torreyana.

Steppes and Deserts of the Western States from the Mojave Desert to the Rio Grande and from ~~the Northern~~ Mexican ~~to Colorado~~ Boundary to Colorado: Arizona: Mohave Agency at the Rio Colarado (Palmer, 1876, nº 523½.!) — Hills on the lower Rio Gila (Torrey); near Yuma (Vasey, 1881.!)

Direction des botanischen Gartens u. botanischen Museums der k. k. Universität
WIEN, III. RENNWEG 14.

Vienna, 3/5 84

Dear Sir,

I am at the point to finish a monography of the genus Ephedra. By the kindness of the directors of the museums at Petersburg, Berlin, Florence etc. I had occasion to examinate a really immense collection of Ephedras. Also the American species were represented very well in it. Nevertheless several questions are still restant which demand to be solved.

While professor Asa Gray, whom I knew personally, is not more alive, I try to address myself to you and to beg you for your kind support. Of course I could have prefered to request you, that you may send me all your specimens of American Ephedras for examination. However I do not know, if you in regard to the great distance, would yield to such a pretension. But you dispose perhaps of a number of doubles, which you like better to give away for some time. The more of it I can see, the

better. Yet if you like to restrain the sending, than the included list shall show you, what I have seen and examinated and what I can miss easely. In the case you have no doubles for me, I restrain my demand to it, that you may look over the list of citations and localities, I could compile from the literature being to my disposition. As the american literature rised in the last years to such an immense extent and a large number of small essays are dispersed in diverses bulletins and transactions of scientific societies, it is for us in Europa hardly possible, to overlook all. Therefore you will find probably here or there some deficiency. You would me obliege very much by adverting my mind to it.

My colleague, D Pilla, demonstrator of our museum asks me to beg you for doubles of Eriophorum Virginicum and Scirpus caespitosus. He is studying on a comparative examination of the genera of Cyperaceas. Perhaps I am also once so fortunate to do you reciprocal services. Be assured, that I shall be certainly always ready to it.

Believe me
yours truly
D Otto Stapf
Assist. at the bot-mus.
of the univ.

HERBARIUM OF HARVARD UNIVERSITY,
BOTANIC GARDEN, CAMBRIDGE, MASS.

May 25th 188 6

My dear Sir,

I have received your letter of 3d inst., in relation to our species of Ephedra, from Mr. Smith of Baltimore. I believe that nothing has been published here regarding them since my paper of Aug., 1883. More material has been coming in, but I am sorry that we have no duplicates which I can send you. You would doubtless be greatly helped in your work by a study of all our collections, and I would willingly entrust them to you did it not seem too great a risk to send them so far.

There are one or two additional species

which I am disposed to believe may be sustained, proposed by Dr. Englemann in a letter of May, 1863.

Mount Joy March 12th 1852

A. Gray Esq.

Resp Sir.

In Your late text book under parasitic plants You Notice a Species of Thesium, discovered by Mr. Mitten to be a parasite in England, And Obsere that "it would be interesting to know if this is the case with our Comandra". It reminded me of the circumstance and on examination I find a copy of a plant culled about six years ago to which is added the following note, "desirous to obtain the entire root, What first Appeared a rhizoma, seemed in reality part of the root of a tree under which it grew, Which I was necessary to detach by force". If this should prove to be the case generally then it certainly will be a parasite. As it is common in this vicinity, I shall, life and health permitting, investigate it more fully - and if it forms Any peculiar attachments, advise you of it should you desire it, I will give you a hasty copy of my drawing.

Comandra Umbellata

Thesium L.

root of a tree

Permit me Also to say a Word on Muhlenberg's Salix concifera, Which name I prefer to Marshall's S. Humilis, though significant Enough, provided they prove to be the same species - You obsere in Your botany "that it often bears a cone-like excrescence on the end of the branches, formed by closely imbricated leaves - probably occasioned by the puncture of Insects", I Am not prepared to object decidedly, but a close inspection of the cone, without any signs of Larvae or escape of exuded juices from the branch - its firm woody attachment to the base of the branchlets, the regularity and in short it looks as natural to me as a regular cone of the pines. I find the leaves variable, stipules toothed. I could notice Nothing like seed - but a uniformity in the texture of the cone as well as regularity. I have various other notes and drawings that I would I could get your opinion upon - hereafter I shall avail myself of your friendly notice, and sent What I find to you should you Encourage it.

Section

I sent You a prospectus of a Work I designed to publish, I was informed then that You had gone to Europe. I have the Manuscript Nearly complete but have not Ventured to incur the expense of publishing it – I have Also Mapped off a botanical chart in the form of a large tree representing the Vegetable Kingdom, supported by the Mineral And its connection with the Animal Kingdom I Branch off first with the Cryptogamia. then through the Algae I come on to the confines of the Animal K. The Phenogamia – Then branch into the Exogens and Endogens, The Exogens Again into Gymno and Angiosperms – Here at the top I separate the herbs from the Shrubs trees &c – those latter I subdivide into Compound – Opposite Alternate And Evergreen – Leaved Orders, Genera or Species, The Herbs simply into Opposite & Alternate – Then all into Monopet – Polypet And Spetalous – here I use such distinctions as are uniform or peculiar, flowers Superior or Inferior (regular or irregular) – Caps. simple or compound &c. &c. availing myself of such distinctions employed by various authors adopting Jeffer Leniners, Lindly & yourself Torry Wood & others as I find Most uniform And decisive so as not to interfere with the too frequent repetition of an Order – seldom if at all a genus. A friend of mine advised me to lay it before the Smithsonian institution – that if they would take hold of it &c something might be made out of it – not fully appprized of the Nature of said institution I addressed a letter to Professor Henry – He sent me a very gentelmanly reply – for Which I am truly grateful, wherein he suggests the propriety of referring it to You or Dr Torry – I know not as yet the Nature of the institution but feel a strong desire to be inducted into its "Modus Operandi" – In short I am often inclined to Abandon all idea of saying a word to the public on Botany and let all my labour go for what I have learned and quietly hunt and Analyze for my own especial gratification – but if I am not Ambitious I'm neither am I Selfish – but wish an interchange a Sympathy and some one to share with me in my pursuit – here I am like Professor Haldiman Who is laughed at for catching Bugs &c I for wading Swamps &c in search of new or rare plants. Then again I admire the Labors of Yourself and Others – In short it is a hobby and I will be astride though it galls me – to think I am like one in a tan Mill, ever moving without making headway – Sometimes I feel as timerous as a country Maiden in a fashionable circle then Again I Assume What I feel I ought to be entitled to the assurance of addressing My Superiors – Excuse this digression – I will not put the cane to my nose – look – Nod – say nothing – and appear wise – If I'm a fool, Well, I can't help it – and with due respect and defference I crave your indulgence for the mood is just in me – And my pen goes like a Locomotive, though it has no clear track before it, Like Stern's (I'll end it.)

Soberly, Dear Sir, If you think that in this Age of book making And Map Making – Changeing without improving – Mine could be easily dispensed with – in candor say so, And I will have it for my children to see what a genious I might have been years ago, You have no idea of the posting up of Jeffers' Centro grade his Natural order – Comparing the Attempts of a dozen to simplify botanical Clasification the tables of Synonomy And placing and replacing – I have almost a cart load of Manuscript – tables &c. Then I have been so long about it – that Mrs A. Wood has got the Start of me before the public – though I can shew a pocket Manuel in Manuscript made eight years ago – but no person seen it – I had no Acquaintances then – and very few now that take An interest in the subject. Writing is no taste to me having served some Years in the recorders office at Phil[a]. only my time is occupied by a Multiplicity of things – to "Keep the Pot a boiling" verry annoying to my "Hobby" – but I have tried to Learn to be content – but like the Irishman's horse learned to live without food – by the time he got used to it, he died – So with me by the time I get my book ready – I'll [illegible] any profit from it. Well I will leave Many drawings – and sheets of Waste paper as a legacy to my Children – If Nothing else –

If you are inclined to Solemn gravity pardon me – for I have written more like to a familiar friend – it is your botanical labors – I know intimately if I have not the honor of a personal Acquaintance – believe me Yours truly Jacob Stauffer.

(10. o'Clock P.M.) I sometime ago sent you a specimen sketch of a Polygonum which you supposed the P. Clinode - Though in flower and erect - I judge you were right - though it grew in a swampy soil - and had a stem much like the impatience - but the leaves & fringed ochre &c were the same. I have yet Another. by way of preface I would state - That a cellar being dug in the neighborhood And the Clay left on a high bank - the yellow clay taken out of the bottom on top Among which a solitary plant sprung up. I noticed it - And in July it flowered I then thoughtlessly culled it for Analysis - My description is thus. Calyx petaloid - col. pale red - 5parted - Stamens 6. Styles 2. Stig. small. Flowers crowded in slender Medium Spikes - somewhat racemed. Stem stout, erect, branching, hairy 4-6 feet high Ochrae striate recurved, very conspicious, Leaves Broadly Ovate subcordate, margins undulate - Acute, petioles hairy - rather ciliate - dialated with a stryate clasping base, - Lower leaves with winged petioles Leaves 2¾ by 2 inches wide - bright green - joints approximate - Ochrae close to each other. a hardy looking plant" - evidently a stranger - Among all my descriptions I could find not one to agree in all essentials. I have never met with Any since like it - could it spring from a germ thrown up with the soil - Are you acquainted with a species that would Compare with my description? - I have an accurate drawing but not the plant itself. Not disposed to quibble and discover new species in this Enlightened age I am ever willing to make some allowance for Accidental Circumstances - And simply call Your Attention to the fact. I Must Now conclude lest I give you a surfeit - Ever with due deference

Yours

J. Stauffer

Prof A. Gray.

J. Stauffer
Druggist
Mount Joy
Lancaster Co. Pa.

Excrescence on the
Juncus. virginicus

Larva of Goodyera pubescens

Description of the Bee found on Goodyera pubescens

Head trigonate, Ocelli 3. Antennae filiform, abdomen sub-pedunculate. Thorax and hairy. hind pair of legs hairy. The female or pair, the hind tibia widened for collecting pollen. It has three submarginal cells on the anterior wings. Like to genus, Anthophora Latr. Insect general rather dark, without metalic colour especially in markings.

Mount Joy July 22nd 1853.

Prof. A. Gray. &c.

Esteemed Sir

After having hastily without especial examination packed the commandra &c your address - While making a drawing next day I noticed several such suckers on the specimen I had reserved (more on account of its fructification than any thing peculiar in the under ground stem) and my attention was then for the first time directed to that feature and the idea you suggested in your note occurred to my mind also - I now send you a specimen of its actual attachment to the root of Vaccinium and as the rootlets are so delicate though I saw numbers so attached the difficulty was to raise both roots so as to prevent a rupture - You will perceive points where attachments were quite abundant and close together and with fragments of the root of the commandra to the root of the Vac - and numbers such "suckers" - This clearly establishes its parasitic character, and as it attaches to the Vaccinium I sent you two species in fruit which by the by I neglected to examine - but they are both low and catible and in fact the same. V. pensylvanicum Lam. & V. Canadense (perhaps) Growing on a dry hill of a clay slate soil - the berries fully ripe, Now I have also a number of specimens in fruit & flower of the Melampyrum ordinary form, which I will forward to you - as I have since not met with the other form, but have duplicates from a former search which I will add -

I will also send you a drawing of the commandra - if of any use - make of it what you can -

The publication I referred to simply contained a statement of Botanical works in process of publication in Europe & America & late discoveries in that branch of Science - in a small pamphlet - The few leave sent me in fact were very acceptable - since I was anxious to know - how my old friend Thos. P. Jones disposed of his Muses &c - I see he has put them to a good work viz. Darlington's Flora Cestrica - Very Resp'y. Yours Jacob Mauffer -

Since writing the within, I stepped into my yard and culled a leaf of Ulmus, Adiantum, Salvia, Rosa – and a leaf of the Comandra & floral & cauline leaf of Melampyrum Laying at hand all within 10 Minutes, collected inked and impressed upon the paper It may not be void of interest to inform you – that I have succeeded in making splendid impressions on Starched paper, and then transferring them upon a Lithographic stone and print therefrom, – it strikes me that it might be of great use for illustrating the venation of leaves the corol, calyx – Bark &c can be equally illustrated and thus the whole flower or plant, and that by a facsimili of the plant itself – as well as other subjects of a flat surface – I have the wings of insects &c also – I submit it to you suggestively – Little things sometimes result in great good –

Very truly Your Ralph E.

L. A.

Mount Joy Nov. 19th 1853

Dr Asa Gray

Dear Sir,

I gratefully acknowledge the page of Botanical information sent me, and am happy in having the greater number of those publications you refer to, The work of Dr Harvey I have never seen. in your excellent work "Plan. Wrightianae" I find frequent reference to "Plan. Fendleri" a work with which I am also unacquainted — Since I take great pleasure in analysis — I have quite a number of exotics which for want of proper works on the subject I cannot name — A Dr Morris of Baltimore gave me quite a bundle of seeds that were not ticketed — I have puzzled over them — and come to the conclusion that I am a very novice in the Science, which however has not discouraged me as a student, for instance a plant which I had placed among my Equisetums — when I came to inspect it closely to my surprize I found Stamens and flowers &c — by the help of Lindly I discovered it to be the "Casuarina equisetifolia" — No doubt brought from New Holland I herewith will risk an exposure of my stupidity in presenting you with a hasty sketch of a plant collected in Ogle County Ill. and no doubt very common, and familliar to you at a glance — Prof. S. F. Baird recognized the plant from a similar sketch which I had with me on a visit lately to Washington City — Though he could not remember the name — he informed me that it was accurately described in your work, Whether he referred to your Manual of the Botany of the Northern U.S. or to some other I can not say — I have searched in vain so far among the Endogenous plants — to which it certainly must belong unless I am greatly deceived in my Idea of that family — I examined quite a number of the flowers and uniformly found 5 Stamens peculiarly inflexed seated on the edge of a disk — the 5 petals also peculiarly inflexed — The 5 keeled ovate acuminate sepals and leafy or chaffy calyx involve more or fruit — seeds I could not detect any — My microscope being no better than a common pocket lence — The whole plant must be at least 2½ ft. high the stock &c. are about 2 ft. long 3/4 of an inch wide tapering to a sharp mucronate point, with close appressed mucronate bristles on the edges, rather distant — But why multiply words — as I am satisfied from my own experience — that you can determine a familliar plant at a glance — can it be that I have hit upon an Anomalous specimen of a Loasaceae — pray have the kindness to inform me. — It is called rattle-snake-weed — but snake weeds are so numerous and Olivers so in character that it is no name at all — But I am credibly informed that it is a specific remedy for the bite of that venemous serpent — and that as they become extinct the plant dies out — this may arise from cultivating the soil in a settled by both — be that as it may — I am curious — and therefore, excuse me for taxing your time and patience

H. Pollard, of Lafayette Co. Mo. has a piece published in the Scientific American, dated the 19th inst — on what he calls the "polar plant" the leaves pointing uniformly to the Cardinal points of the Compass, and considers it "designed by the great Author of Nature, as a guide to the pathless traveller through the prairies" &c. he also calls it "Rosin weed" Can he refer to the Silphium laciniatum or S. terebinthinaceum — he says "A kind of gum, in smell and appearance resembling turpentine, exudes from the stock"; hence the name of "Rosin weed" which it bears in this Country. — His latter name you apply to the S. laciniatum — I never before heard of the name "Polar plant" I have Specimens of Silphium and hence desire to know — The writer is evidently no botanist — for no clue is given as to the generic character of the plant. — To conclude, I shall follow up your hint and ferret out the parasites, at time and opportunity offers — and make my self thoroughly acquainted with Systematic Botany, if I can — had I but a wider range, and access to the proper means for investigation.

Hoping you will excuse my lengthy Epistle — I shall be content with as brief a notice as your time will afford — I have numerous many questions to ask — besides — with which I will not annoy you — and at the present time believe me Yours

truly gratefully

Jacob Stauffer

N.B. have you duplicates of southern & Western plants that you could spare — I'd cheerfully pay the freight by express

Rattle snake Weed.
collected ... in ... Sept.
in ... Ogle County, Illinois
From a Dry Specimen

Mount Joy Pa. Dec. 1. 1841

Prof. D. C. Eaton

Esteemed Friend

After a long silence suffer me to give you some knowledge respecting my own affairs, and that will I trust be a sufficient reason.

Having had difficulties enough of my own, to keep me in hot water I nevertheless endorsed a wealthy neighboring farmer at Bank, which to my surprize as well as cost, the friend was sold out by the Sheriff for note protested and to avoid the same fate, had to make an assignment, and sell my Drug Store at a sacrifice, and thus I was crippled at all points.

Rev. J. G. Morris of Baltimore loaned me 21 vols. of Sherwin Flora beautifully lithographed and faithfully colored, which plates, about 1200 in copied, as well as those found in your own works, together with as many more from plants collected by my self or sent me.

Being skilled in perspective, sketching, scenery, Animals, flowers &c. of an indefatigable temperament, naturally disposed to diligence and persevering to overcome difficulties, age 26, if shall see I could be well qualified to make one of a party for an exploring expedition.

The question is, could I not find an opportunity to gratify my taste, at such a salary that would enable me to sustain two children at a boarding school and a prudent economical wife at home for a year or so.

Your position and standing might be of service to me in such an event, provided you deem my skill and experience in drawing sufficient to warrant your recommendation from what you may

seen, I might have specimens and drawings of many rare & superior & new
(of course)
or have seen, Butterflies, Moths, Beetles in short insects, Hy correct and
birds pronounced back to the life.

My eldest son is of age, has an excellent Job Office and edits the
Mount Joy Herald, a respected Weekly paper for the County.

At present, I letter and print signs, make wood cuts, and (last week) make a drawing of the Mount Joy Cap Factory, for the purpose of having it lithographed, in four colors. This occupy my time and contribute an article on Botany, occasionally illustrated by one of my wood cuts formerly engraved with a view of being published in a Book.

We have now reached No. 40. When the Year is up I shall discontinue it, as I have no longer any profit interest in the paper, save what arises from a desire to see it prosper.

You will pardon me for troubling you with the foregoing, but as its purpose of giving you an idea of my position and the desire I be engaged in a field more congenial to my taste, I am willing in some hint or advice that might be to my advantage since I barely make my daily expenses though moderate and easily satisfied.

Any illustrations or Botanical information will be ever gratefully

Received by return mail. Very

Yours &c.

(4) H

Prof. Asa Gray
Harvard University
Cambridge
Mass.

Monterey Oct 27 1855.

Dr A. Gray

Dear Friend

I have a few hasty sketchings for your opinion with fragments of the original plants.

I may at some future day send you perfect specimens of each - as they are not rare with us along Chicques-creek.

I often wish I were near you. I have so many questions to ask - which no doubt are troublesome enough to you as it is - charging you three Cents for every answer. That is not the way Attorneys answer questions - But alas Botanists are poorly paid - government ought to allow them the franking privilege -

Not as those who pursue the study of Botany are not blinded with the glare of gold - are also void of that three cent selfishness - if indeed not absolutely too poor -

No. 1 on the South of Mount Joy on Rocks near the little Chicapee Creek. No 2. on the Rocks near the same Stream North of Mount Joy — Belonging them one and the same thing Comptoniacrous Myrophyllum Lin. I made no examination till taking a drawing when upon comparison I found the peculiar difference in the fruit dots — the above are the nat size of 2 Specimens. though common in this locality — I unfortunately have but 2 Specimens in fruit — of each, which I covered with printers ink and took impressions of. I will enclose you parts for your inspection.

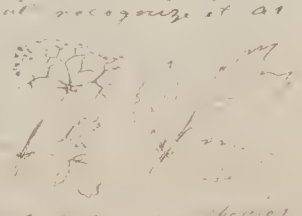
You say — "If Antigramma be not a distinct genus, it must be reduced to Camptosorus, which is the oldest name!" — I have no account of Antigramma in any book in my profession; and hence know nothing about it. Therefore I submit it to your examination for further light on the subject — I have as yet been unable to procure Lindley's Vegitable Kingdom.

Send me a Specimen of Scolopendrium officinarum — N. New York if you have one to spare. I compared a European specimen of Asplenium one Trichomanes (minus no 1. with our species no 3 — — — there is more analogy with our A. ruta-muraria fig 2. in the disposition of the fruit dots — though alike in habit and growth in the general form — very truly & great respect your obedient humble Servant

Jac. Stauffer

Eastwoodia

Pentstemon



Mount Joy Pa. Nov 26 1857

Dr. A. Gray

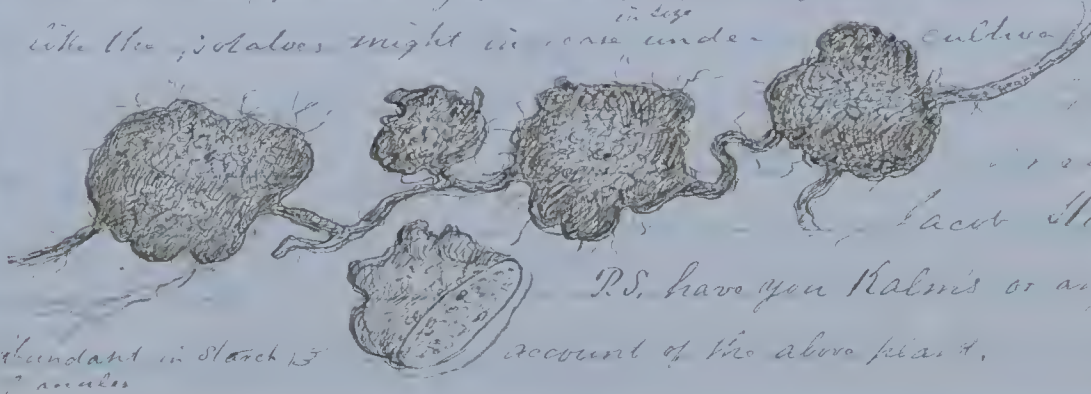
Dear Sir

I am truly thankfull for the several tracts sent me, your "Halistics of the Flora of the north. U. S." interests me greatly. I hope that you will ever remember me, I preserve them all with a view to having them bound in a book &

I have been writing for a year past, for several monthly publications on subjects connected with agricultural Botany and Entomology. I enclose you a proof sheet one of an article furnished.

As to the plants wanting in my collection, I fear the number is greater than I at first supposed, but I will not now annoy you respecting that subject. When I know what I really want I will send you the list.

I desire all the information I can get from others respecting the Apios tuberosa - I a few summers ago took up some of the roots with a view to test their edibility and found them sliced and roasted equal to potatoes. Darlington says "According to Kalm, the Indians and Swedes called the plant "Hopniss or Hapnis" and also tells you he admits the tubers to be esculent. - Under cultivation they might be made as valuable, perhaps, as the Dioscorea &c. Have you seen the tubers? I will give you a copy from a specimen found in a wild state, they like the potatoes might in some degree be improved in size under cultivation.



Abundant in March & April

I would like to have your account of the above plant.

Yours &c

Jacob Mauffie

P.S. have you Kalms or any other persons account of the above plant.

P. S.

Dear Sir

As I design exploring the Alleghanies next Summer
Perhaps a line recommendatory from you might be of
service to me in procuring a pass over our rail-road-line,
as the expense of travel is a serious drawback on me.

Your character is established, and therefore (if no scruples as to my worthyness of it prevent,) you can aid me in my views.

Very truly &c.
P. S.

Lancaster Dec 12 1882

Prof. A. Gray

Dear Doctor.

I undertook to investigate and ascertain the cause of the production of the "Cedar Apple." last May and June by Hedgcock and Hunt, but failed to find the gelatinous Excrescences formed (after a rain) on a few shrubs of the Red cedar (Juniperus virginiana) Fig. 1 shows the yellow gelatinous appendages arising on the orange colored outside of the "Apple," Fig. 2. one cut, showing the dense texture b, of the interior, of white color, closely investing the radiating tissue. Is it a gall? or a fungoid production? I examined the exterior in various leaf branchlets, at various periods, and never could detect any larva, until late in Nov. and Dec 12th day of Dec. out of eight "Apples" carefully examined three contained each a larva of a species of Lepidoptera, allied to or of the family Tineidae, similar to the Apple worm, they do not produce the excrescence, but use them as a nidus. I have specimens of the larva which will develope the imago by next Spring. Curtis and Zeller described the larva of this family found in "Apples" in Florida and Brazil and Cyprus. Prof. Mr. Mac Cay I presume saw the cocoons. Both Mr. Curtis & Zeller were acute observers, and could see by the naked eye, I think, between a gall or a cocoon. But as Prof. Zeller judges the exterior formation to be a fungus - I am not yet convinced although it has been shown by Tulasne and others, that the external fruit of species of fungi, belong to the true Fungi; and are not mere anamorphoses of the cortical or epidermis, which assume the appearance of fungi, as in various galls produced by insects. Because of the compact texture of the excrescence being formed first, and the yellow jelly-like exudations appearing from it on after a shower, do they be fungoid - I could not detect mycelium or mycelium Mixed in the gelatinous yellow masses. it is true that a shallow circular rift is found in the cedar tissue after they are dried up and fallen off - as shown by Fig. 3. - Fig 4. is a cut section of the same as Dec. 12. Fig 5 is a dry apple - the interior is of a reddish brown - stringy fibrous tissue, with a pale cream texture. without trace of a larva having been in one, after careful search - they are similar to those others, some of which I put into vials and obtained the perfect insect - I propose - I know not to preserve it will the color change it may undergo in its transmission?

[illegible] nature of these [illegible] contended [illegible]

Owen [illegible] Worth [illegible] that you can decide the question, at any rate, an [illegible] to [illegible] Physiology.

Fungi are found usually growing upon decayed vegetable matter, except the lower groups, such as the Lichens, these are found more commonly on the bark of living trees. Some writers have questioned whether such productions are not mere fortuitous developments of vegetable matter?, called into action by special conditions – considering their sudden appearance (the pollen of [illegible] supposed to) perhaps Meteoric, after a Thunder Storm, Being also confined to the same limb or tree, But as Fries, remarks, that the sporules are so minute, in some instances, that 10.000.000 or more [illegible] eye – It is a question that I am not prepared to solve. whether fungi so called are sometimes of such a nature, as to incline to the opinion advanced. Be that as it may – I will simply add – that I have been long familiar with the "Cedar Apple", as an object of mine but am not aware of any published opinion as to their nature of growth. It has not [illegible] if any, astringency – a slight Terebinthine taste, is all I could detect, and being recommended in small doses from 5 to 20 gr. three times a day – I can not suppose it very active, nor related as a vermifuge – I will now refer to another subject –

Last Summer I had growing in an Air Tight among Moss, Kept moist, a few Specimens of the [illegible] the height of 18 inches – it flowered freely and fruited – and is alive yet. [illegible] there an [illegible] insects, led me to inspect this [illegible] – I observed [illegible] embedded in the [illegible] of the flower on several occasions, and captured one – which I judge to be a species of A small Bee. Apidae belonging to the third class or Solitary working Bees – next Andrenidae. Megachile-like) have collected pollen from plants, – for want of a description of our Genera of Apidae I can not venture upon a generic name of the little Bee in question –

Other pursuits interfering – I did not succeed in making more extended examinations, [illegible] agreed to [illegible] upon an investigation with the next summer we is here the [illegible] Flora – as you are aware – having full knowledge of your [illegible] experience [illegible] and kind epistle – I said more than I intended – Happy to see [illegible] to talk upon [illegible] about the "Cedar Apple" Yours [illegible]

– Jacob [illegible]

Lancaster June 9th 1864

Respected Friend.

I concluded to communicate to you a fact brought to my notice, in Vegetable physiology perhaps rarely witnessed. Mr. Daniel Engle, a man who takes great delight in the Orchard, and a prominent Member in the Fruit growers Society of this county, had a thriving Pear tree, a branch of which was torn during a Storm in January; he tied the broken branch up, it soon united, and put out many branches and made from 12 to 14 inches of young wood during the remainder of the Summer, & in March of the following year, on examination, he discovered he found the wood had [illegible] a full inch below the granular formations on the stump of the tree and that formed on the branch, yet the branch was fresh and vigorous.

With out reflection he cut it off - as rather unsightly, and as a curiosity brought it to S.S. Rathvon of this city, who handled the branch to me, of which I took an accurate drawing and made a memorandum. It is evident that all the sap between the point stem and branch, the circumscribed [illegible] as in all cases. The granular excrescence may have had the office to prevent moisture from the wood, which are disposed to throw out in lots, and if imbedded in clay, kept moist, or the humid state of vegetation, would finally take root and allow a development.

I will append a few reflections, on this matter, of course they may not be correct, still it is by drawing various suggestions, the same time investigating the corresponding laws. The vital nature is as skilful in restoration as in growth - wonders upon wonders of which I adduce the above an apt illustration. The vital forces of the tree are soon to repair the injury and concentrate their powers upon the nearest possible point to the seat of the injury, or the extreme line of demarcation between the sound and broken parts.

It is not alone by the agency of the sap, that the life forces of the tree are kept in circulation, but the imponderable element of vegetable Od, or if more agreeable, the [illegible] particles of Electricity, which ever clothe and protect and direct the [illegible] restoration, (away in case of injury [illegible] [illegible]

out the ... to flow from opposite directions on the line of nature, as if to ... the diseased part.

The gross electrical current, then ... acquired activity by the new ... upon its powers, flies, as it were to fill the vacuum, or to keep the point of discipline and pursue its natural course until the "od" reaching out with its strong attraction, comes so nearly in contact, in opposite directions as to cause a ... flow of the fluids, and we may see (as it were) the fine streams of electricity flowing, or ... across from side to side. Here is something on the principle of ... flowing from one cloud to another less highly charged.

Now, if these conditions were observed and surrounding elements ... closely to the wounded parts, there could be no difficulty in restoring the neutrals ... as the case described. When there is enough of the bark ... fine or lexicon to ... operation. Take a narrow strip of bark, from any tree of the same species, ... bridge or staple (resting the outer edge of this bark and thus making it ... ends. Then make a mixture of finely ... and burnt loam sprinkled ... the mixture a handful of iron-filings & stick forming the roots ... into the mixture. Or in many cases the bark operation may be ... united with an exposure will be formed gradually extending from ... till a new covering of bark will be formed.

This again on the same principle that a severed artery will put out a new section ... below the communication.

Some of the ideas may seem rather to smack of transcendentalism, to you, or rather ... relative to other wise; it is my object to draw with your critical eye, and candid ... , consideration that I have learned from the external resemblance of things to the ... true nature, and find the perversities in the invisible or imponderable ... to be ... That Revelation for Science is to many phenomena, or otherwise ... natural action.

I am Respectfully Yours

... Stauffer.

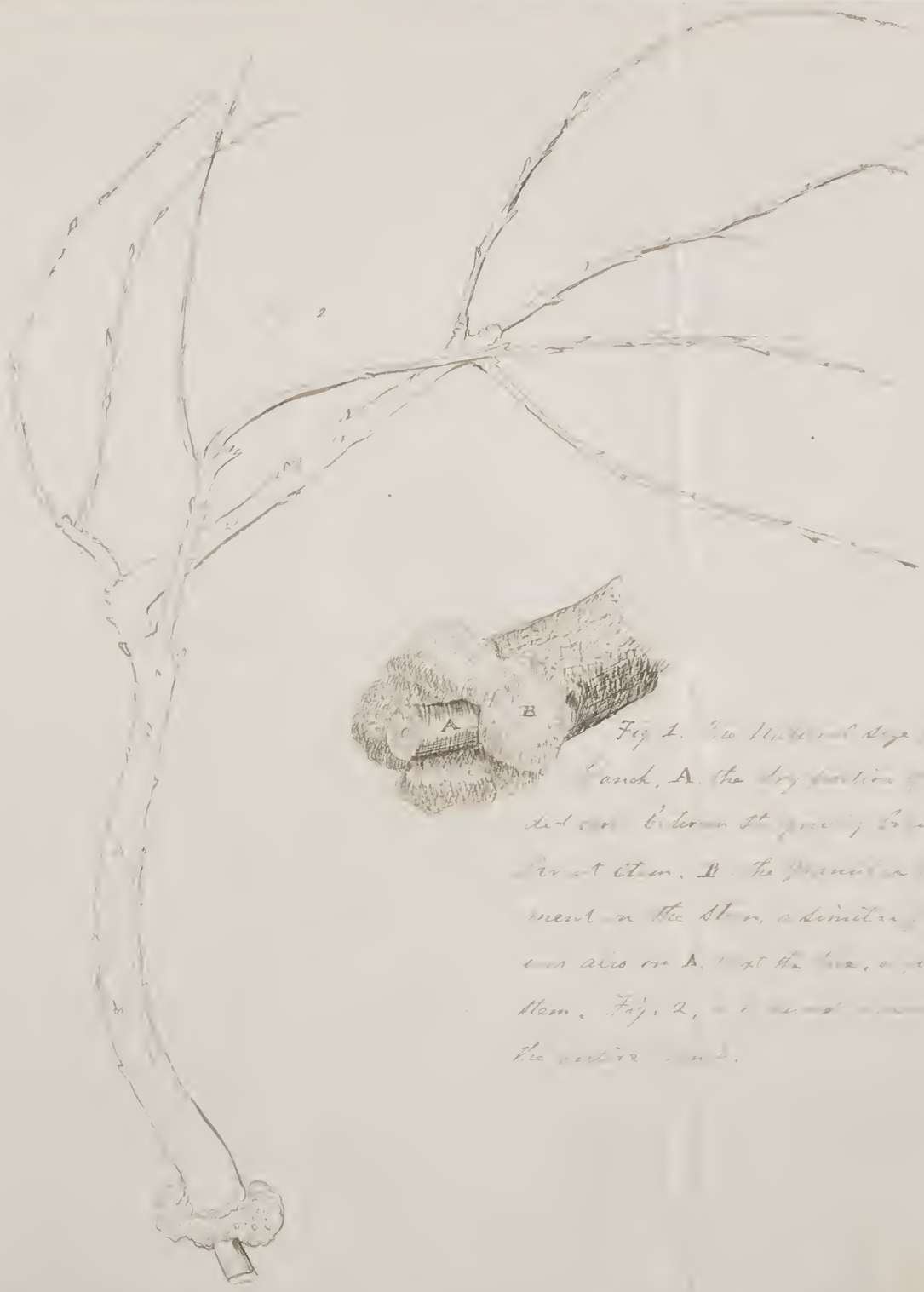


Fig 1. The ... of the branch, A. the ... divided or thrown ... Natural section. B. the ... present on the stem, a similar ... exists on A. at the ... stem. Fig. 2, ... the entire ...

Copenhagen

(the 1st July 1857.) the 25th September 1867.

My dear Sir!

An illness of several months and the consequences of that illness is the only reason I can allege for not having answered the several questions in the letter, with which you honoured me. I shall now however, if even late, endeavour to answer them as in duty bound, — but previously beg to return you my thanks for the said letter. —

Let me then first inform you, that our mutual friend, Professor Liebmann, whose state of health you so compassionately enquired about, after a years almost uninterrupted confinement to a sick-bed ended his sufferings last October. Science thereby lost one of its zealous cultivators, his friends lost in him still more. As I came daily, often several time a day, to him during his illness, I communicated to him your friendly enquiries as soon as I had received your letter; he was much pleased to hear from you and wished to write to a letter to you himself — How far he really accomplished this, I know not, as I was for a long time, by my own illness, prevented from seeing him; but if you have not received any letter from him, then I have many kind remembrances to you from him, which I here sincerely bring you. He was, until the last moment, in full possession of his mental faculties, and spoke to the last about his friends with several of whom he took a written leave some few days before his death. — How his place here at the university will be filled, we know not yet; but it is most probable that the many lectures will demand, that the place shall be divided in two different. — You know, that we have two competitors for the post, Lange and Forchhammer, the monographical works of the latter are known to you. —

I [illegible] Lange 2-3 months younger than Forchhammer. The latter is a good deal more practical and has more talent for lecturing. I for my part prefer Lange, but I am not sure that the majority of the faculty will not vote for Forchhammer. Both are excellent men, and I am sure that either will be a valuable acquisition to the University. —

I have received your kind letter of the 1st July, and thank you for it. I am glad to see that you are in good health, and that your family is well. I am also glad to hear that you have been enjoying yourself in the country. I am sorry to hear that you have been ill, but I hope you will soon be well again. I am glad to hear that you have been reading my book, and that you like it. I am also glad to hear that you have been writing to me. I am sorry that I have not been able to write to you before, but I have been very busy. I am glad to hear that you have been enjoying yourself in the country. I am sorry to hear that you have been ill, but I hope you will soon be well again. I am glad to hear that you have been reading my book, and that you like it. I am also glad to hear that you have been writing to me. I am sorry that I have not been able to write to you before, but I have been very busy.

It will certainly not be without interest to you to learn that of Liebmanns' works there will at least be completed:

1) the 43rd fascicle of Flora Danica, the plat. for which, until the last third-part were engraved before his death. —

2) his Atlas over the tropical — american oak (Quercus) of which 40 plates were ~~find~~ engraved in October, when he died, and the engraveing of the others is now continued daily. As his nearest friend I had given him the promise to have the superintendance over the publication of these two works, but Mr. Lange, the author of a Synopsis or Manual of the danish Flora, will have the botanical department of the ~~Flora~~ that fascicle of Flora Danica; he also will be the continuer of this great work, and, as we all hope, its completer (7 fasciculi only remain, about sixteen years work).

What I shall do with Liebmanns' great work on the Palms, I know not yet determined; there rests a great responsibility on me in that respect, for my deceased friend placed this work solely and entirely in my hands — or more properly speaking on my heart, for whatever I determined about its publication or non-publication should be settled for him and his. Perhaps you are aware (I know from Liebmann himself) that almost all the plates (59) have all copies been drawn for ten years or more —

So much about our lost friend!

As to the continuation of the scientific communications from our Museum of natural history, I shall assuredly take care that they be sent to you, and you will probably have long since got the second part for the year 1854, as you soon will receive the communications for the two years 1855 and 1856, which both have been sent to Leipzig for you in the packet to the Smithsonian Institution. I trust, that the complet series from 1849 have all come safely to hand: I had also enclosed Lange's catalogue of Greenlands plants, as I thought it might have a special interest for you.

As soon, as the work on the oaks is completed, I shall take care not to forget that a copy be sent to you in Liebmanns name, and likewise of his work on the Palms, if this in the event of its being publicated (as I hope). —

Since my removal from Sorø, where I was Lecturer in Botany at the Academy, I have not specially occupied myself with that delightfull science, but have kept more exclusively to Zoological and palæological studies. I have nevertheless an Interest in botanical Papers, and a very great one when ever they relate to the morphological &c. or to the biological side of the Plant [Vide, that Alex. Brauns treatise on the individuality of plants, its first part, is given in your American Journal, I hope, that I shall still may be able to express myself more explicitly respecting my views of the individuality of plants, which I still believe are the more correct, and in this respect I take the liberty to observe, that mine are not perfectly given in Brauns book, as more authors have observed] — I will not deny, that zoological treatises have in general greater interest for me, as they enter more particularly into my present chief-studies; as I am a great friend of literature, such copies of American treatises as you mention in your letter will consequently be extremely acceptable to me. Through the obligingness of Prof. Agassiz I possess several of his excellent treatises, namely those of the Medusæ and Berids. — The proceedings of the American Academy, which you mention, would indeed be extremely welcome, especially if I could have a prospect of seeing the series completed.

Are you in possession of Wahls Elog. Americanæ, or of Humboldt and Bonplands Nova genera (vols I, II, III), or of Lyngby Hydrophyto-logia Danica? if they, or some of them can have any special interest for you, I will enclose them in the next packet for the Smithsonian Institution. but I must them beg to be informed thereof by a post-letter. I remain

Yours sincerely Japetus Steenstrup.

Copenhagen
University Museum.
Feb. 22th - 1861.

Steenstrup.

Dear Sir!

In a box ready to go with the first Bremenian - Steamer to Washington are put two Folio-Volumes (viz: Vahl. Icones Americanæ and Symbolæ) and an Octavo (purge) to You, the last containing a Book from Prof. Johan. Lange -

In your next box the Vedensk. Selskabs Mindeskrift for 1860 and some other parcels will be found.

Of Your Proceedings of the American Academy of sciences and Arts I have received vol. I. II. III and IV (1847) to page 248. -

I hope the Mindeskrift (Mindeskrift for) 1859. - in Your hand

Sincerely Yours

Japetus Steenstrup

Professor Asa Gray

Cambridge

Mass.

U. S. of America

R 2. L

Japetus Steenstrup.
Nørregade 10.
KJØBENHAVN. K.

the 20th December 1879.

Honnered and dear Colleague!

Unhappily there has been more than one trouble of the wandering of our Videnskabelige Meddelelser fra den Naturhistoriske Forening to the western world during the last two or three years; but we hope that all shall be reversed and in the future go quite regularly by poste, if possible.

So the first halfpart of the publications for 1879–80 has been sent by poste to every one of our connections and I hope it in Your hands, dear Colleague, at least a week before this letter.

The volume containing the communications for the years 1877–78 has been sent to the Smithsonian Institution's Agent (Dr. Rud. Lincke) in Leipzig in a great Parquet, adressed plainly to the Smithsonian Institution and before long I hope also this volume in your possession. —

The first of the volumes You were wanting, when writing the Letter of 1 Sept.

Immediately after that time only some few copies of the publications

namely for 1876, has been sent the same way 1½ year ago; it was a rather great volume; if still it is not arrived, I beg You ask for it in Washington, and if not there, I shall immediately reintegrate another copy and that via direkta to You. Becoming now near the end of this year, I beg You receive my best and sincerest wishes for the new Year!

Truely yours

Japetus Steenstrup

Postscriptum.

Copies of 1876 were in the same packet to Professor Agassiz to the Museum of Comparat. Zoölogy; if these have arrived to Cambridge, then I think not worth asking in Washington, from here to Washington they must all have the same fate or consignment, if they have not been despatched.

To Professor Dr Asa Gray
Harvard University
Cambridge · Mass.

Copenhagen. Naturhistorisk Forening.
24/8 80.

As the parts of our "Videnskabelige Meddelelser" for 1876 & 1877-78 have not yet been received, is it to be feared that the sent copies will not arrive, a new set for the years 1876–78 we shall therefore send the best way possible. —

J. Stp.

BREV-KORT.

(Paa denne Side skrives kun Adressen.)

DANMARK 10 ØRE PORTOFRIM.

Til

Professor Dr. Asa Gray
Hochwohlgeb. Harvard Univers.
Cambridge
Mass.
U. S. N. A.

Hamburgh, July 24th 1850.

My dear Sir,

I had the great pleasure of receiving your kind favor of June 4th some weeks ago, and I return to you my best thanks for it, so much the more, as I see, that you had the kindness to induce my old correspondent Dr Short, to recommence the interrupted correspondence with me, by sending me a very acceptable parcel of North-American plants, which reached me the first days of June, but about which I received the letter from Dr Short, dated the 26th of March, not earlier than to day.

Most of all I am obliged to you, that you will have more over the kindness of communicating me some plants of New England states, for, as I learn in your excellent Manual of Botany, ~~that~~ I am wanting in many species of plants from that part of your rich Country, even in some genera, and samples examinated by yourself, are as well as Original-specimens from the author. — Therefore they will have the greatest interest for me, as I am very anxious of adding the names of plants to my herbarium which are not identified by myself with the description or drawing in the classical Floras

of every country. Nevertheless by comparing the best and accuratest descriptions, which however you have published in your most distinguished Flora of North-America, sometimes a doubt is not to be avoided, and in such disappointment nothing can help than the autopsy of an authentical specimen. —

Therefore it gave me the greatest pleasure that you informe me of your travels in Europe, and I am very desirous to hear of your safe arrival in our Continent; for, if I do not mistake, I think, you instead by the long stay in Europe, to collect critical materials for the continuation of your great Flora of North-America, which is desirously expected by every botanist, who knows the critical worth of your most valuable work; and indeed I am very happy, that you let me hope to make your personal acquaintance in my own house, where you will kindly look into my double full species of North-American plants. I shall be very glad, if I could do anything for you, to make your residence at Hamburgh as agreeable as possible.

I repeat to you my best thanks, particularly for the copy of your Enumeration of Roxbe Lindheimeri, and, you had the kindness to send me, which however has not arrived; but the ship is expected every day.

I beg you, my dear Sir, to accept the assurance of my highest esteme, with which I remain

Yours very truely
Joachim Steetz Dr.
Pachrwieder N. 30. Hamburgh.

Hamburgh, the 12th April, 1853.

My dear Sir,

The inclosed Diploma as honorary member of the "Naturwissenschaftliche Verein" at Hamburgh, by which our society wishes to give you a proof, that your eminent merits on science, particularly on Botany, are fully valued and acknowledged by us, and which we beg you kindly to accept, reaching you so late, I trust you will excuse, for I was prevented by a dangerous illness, a pneumonia, which befell me last autumn, and kept me at home for several months; the succeeding weakness interrupted all my correspondence. Now I am glad to say, I am quite recovered and am beginning to take off my debts thus incurred by writing again to my literary friends.

I avail myself of this opportunity, to return to you my best thanks for the copy of your "Plantae Wrightianae", which you had the kindness of sending me, and which I had the pleasure of receiving several weeks ago. It has the greatest interest for me, being a work of most scientifical value.

With the same appreciation I read your fine exposition and descriptions of New Holland Compositae, which you had published in the London Journal of Botany, 1851 and 1852, and I congratulate you upon it. When I not agree in your views in reducing my Pogonolepis to Skirrhophorus, and restoring the Asteridea of Lindley, I nevertheless respect each perception, which is founded on arguments; for my part I should think, that Pogonolepis is better distinguished by three constant characters from Skirrhophorus, than Asteridea from Athrixia hardly by one; particularly as Athrixia Ker. is compound of several species with the same habit, but with very different pappus, and if you had admitted the other Australian species, in the same genus with a third and fourth section, you would have a similar genus as your own Skirrhophorus, chiefly as Lindley wrongly placed his genus Asteridea in the Tribus Asteroideae, Subtrib. Inuleae. On the other hand indeed I dont know, by which other decided character Athrixia australis could be distinguished from Athrixia Capensis Ker. and A. phylicoides Cand. than by the whitish colour of the ligulae, which however is said to be the same in Athrixia debilis Cand. of Madagascar, which I have not seen.

My friend, Dr. Sonder from here, who received a great collection of N. Holland plants from Port-Adelaide has just now examined a great number of them, of which the Compositae, I believe more than 180 species, will be published in the next number of Schlechtendal's Linnaea. Many new genera, and

many new species of your new genera have been found amongst them, and also a new Pogonolepis, some new Leptorrhynchus and the true Pycnosorus globosus Benth. which we have compared with the original specimen of Bauer in the Imperial Herbarium of Vienna, and another species of this interesting genus, formerly described by Schlechtendal in the Linnaea, under the name of Calocephalus? chrysanthus. Bentham however has erred, in finding one neutral flower in each capitulum. Sonder states, that all flowers, even in the original specimen are truly perfect. I hope, Mr. Sonders exposition will be of some interest for you.

I much regret, that in the same time, when your descriptions of Drummonds Compositae were published in the London Journal of Botony, my friend Turczanin, now described the greater number of the same collection in the "Bulletin de la societé Imperiale des naturalistes de Moscow. I communicated to him the collection in 1851, but it was too late, and the second series of his "Synanthereae imperfectae" were already under the press. Without doubt you have already seen a copy of the numbers of the "Bulletin" in which they are contained. In his answer from the 13th March, 1852 he writes thus:

"C'est bien dommage que nous nous sommes rencontrées avec Mr.
"Asa Gray dans la description des Asteranthées, d'autant plus que
"je n'ai reçu le "London Journal of Botony", que j'argue au mois de
"Juin 1851. Il paroit pourtant, que ma premiére brochure sur les
"Conposées est imprimée avant celle de Mr. Asa Gray. Ma Gnephosis
"arachnoidea est bien Nematopus effusus Gray, et puisque cette plante
"n'est pas une Gnephosis, elle doit conserver le nom de Mr. Gray,
"seulement je trouve une couronne très etroite, qui se detache facilement
"de la graine et qui est son pappus, tandis que Mr. Gray assure, que pappus
"est nul. Vous pouvez verifier cela, puisque je vous envoie l'echantillon
"de cette plante. Vous avez très bien reconnu les deux Cephalosurus
"de Mr. Gray, dans mes deux plantes, qui ne me paroissent pas congeneres.
"Il faut ajouter à cela, que ma Gnaphalium flexuosa est Dithyro-
"stegia anapleuricaulis Gray, et que l'involucre partiel est veritablement
"une grand écaille enveloppant les fleurs, que j'ai trouvées solitaires.
"Je l'ai pris pour un sec, qui ne s'avance qu'au sommet. Mon Gyrostemon
"thium rhizocephalum est Chamaesphaerion pygmaeum Gray. C'est dom-
"mage que Mr. Gray n'a pas mentionné les numeros des collections!
"Il seroit plus facile alors de comparer ses descriptions aux plantes."

The capitula of the specimen of Turczaninow's Gnaphosis arachnoidea (your Nematopus effusus) are not at all developped, and the achaenia not mature, but as I believe, the achaenia are truly epappose, and Turczaninow took the increased base of the corolla for the "couronne etroite, qui se detache facilement de la graine". I much regret, that I have not Mr. Drummond's collections, which are much for more extensive than the collection of Preiss. Mr. Turczaninow kindly sent me a few species of Drummond's Compositae, of which the following ones may be about synonyms with yours.

Gnaphosis arachnoidea Turczan. Drum.	Coll. 3 N. 120	= Nematopus effusus Asa Gray
—— macrocephala Turcz.	Coll. 4 N. 202	= Cephalosurus gymnocephalus ? Gray
Piptostemma compositoides Turcz.	Coll. 4 N. 200	= —— phyllocephalum ? Gray.
Chrysocoryne cuneifolia Turcz.	Coll. 3 N. 116	= ? Chrysocoryne myosuroides ? Gray.
Schoenia chlorocoppa. Turcz.	Coll. 4 N. 197.	
	Coll. 4 N. 204.	Ann. Chthonocephalus Drummondii Asa Gray.
Helipterum pusillum Turcz.	Coll. 5 N. 384.	
Acanthocarpum filifolium Turcz.	Coll. 3 N. 119.	
Helichrysum fuscescens. Turcz.	Coll. 5. N. 64.	
—— heterocarpum Turcz.	Coll. 4 N. 214.	
Podolepis Gilberti. Turczan.	Coll. 5. N. 386.	Ektne Panaetiae Cass. 2 ?
Gnaphalium sericeum Turczan.	Coll. 5. N. 392.	= Gnaphalium indutum. Hook. fil. ?
Pachyurus multiflorus Turcz.	Coll. 3 N. 117.	
Argyroglottis turbinata Turcz.	Coll. 5 N. 63.	
Angianthus tomentosus. Woodl. ?	Coll. 4. N. 217.	Prevera videtur!
Schizochorus Drummondii Turcz.	Coll. 3 N. 123.	Turczaninow remarks on the Etiquette: "Later species a cl. Gray descripta "haec non recognovi."
~~Crassa~~ Myriocephalus ? villosissimus Turcz.	Coll. 5 N. 62.	= Trotolepis eriocephalus Asa Gray.
Epitriche cuspidata Turczan.	Coll. 5 N. 53.	
Ozothamnus leptodes. Turczan.	Coll. 5 N. 385.	
Waitzia decolor. Turczan.	Coll. 4 N. 198.	= is my true Waitzia acuminata !
Toranthes major. Turczan.	Coll. 5 N. 53.	= Leptocoronis vescosa. A. Gray.
Trichocline graminifolia Turczan.	Coll. 4 N. 207.	(A good name !)
Trichostegia asteroides Turczan.	Coll. 5 N. 66.	= Asteridea multiceps. Asa Gray.
Chrysodeixis nivea ? mihi seems to be your true		= Asteridea stricta. Asa Gray.

I beg you, my dear Sir, if it should be possible, to complete this little list, and to communicate to me in your next answer your own Synonyms. Therefore I added the original N.° of Mr. Drummond's collection.

In the same letter Turczaninow begs me repeatedly, to send him Dionaea muscipula. Ellis. But I have no more than a morsel of this rare plant in my own Herbarium. Therefore I would be very grateful to you, if you could procure me some instructive native specimens of it.

I have lately written a little treatise on the family of Frenandreae, and its natural affinity to the ordre of Lasiopetaleae, which is called forth by Mr. Payers article in the "Annales des sciences naturelles" de 1851, on the "Organogenie de la Corolle des Polygalinées", and as soon as it will appear, I shall have the pleasure to offer a copy to you. — You had the kindness in your last letter to announce me, that you have destined for me the second part of "Plantae Lindheimerianae", which I should receive by the care of Professor Lehmann, but I never received it. The first part of that valuable work I got from the Bibliotheca of my deeply lamented friend Prof. Zuccarini at Munich.

I learned in the Journal of Botany, that you dont know my Chrysodiscus niveus and the two species of Silphiosperma, and I have the pleasure of reading you a specimen of them, and you can judge yourself, if the former is the same as your Actinidea stricta. I much regret, that they are not in a better condition, but I have no better specimens in my own herbarium, and no more duplicates. I hope however you will recognize them by your great acquaintance of New Holland Compositae. I added some but very few other Compositae, a Tetratheca pilosa var. ? Hooker from Port-Adelaide, and an interesting variety of Myriophyllum verticillatum L. which has no other than female flowers. I dont know, that such a variety has been mentioned by any other botanist. I found on the same spot no other specimens, either in summer, or in the last days of autumn. — I beg your pardon in offering to you such a trifle, but by the great extension of your rich Herbarium, it will be almost impossible to me, to send you anything, which could be of some interest for you, but I should be very glad, if you would be able to name anything you wish for, and that is in my power to send you.

I much deplore, that the hope you gave in your letter of a visit to Hamburgh during your last travels in Europe, has not been fulfilled and in that way I have been deprived of the pleasure to make your personal acquaintance.

With the highest regards I remain, my dear Sir

Yours truly
Joachim Steetz Dr. med.

pr. Addr. Rothenwieder No 30
at Hamburgh,
or Care of G. F. C. Röding
Holländische Reihe No 13.

Ansd. Oct 4

Hamburgh, September 6, 1853.

My dear Professor Asa Gray

Your kind and so detailed answer was very welcome to me and most interesting in every point; it reached me a few days ago, and I lose no time in rendering you my best thanks for it, as well as for your most valuable Plantae Wrightianae part 2, Plantae Lindheimerianae, part 2. and Plantae Fendlerianae, which you had the kindness to pack up for me.

I am sorry to say I never received the Plantae Lindheimerianae, nor the Plantae Fendlerianae. My friend, Dr. Klotzsch at Berlin had the goodness to lend me his own copy of the latter work, as I required it in working up the Compositae of Panama for Mr. Seemann. To avoid a similar occurrence I beg you to address any further communication direct to me: Rehrwieder, 30, or to send it through Mr. Röding, who never fails to deliver it immediately; but never again through Prof. Lehmann.

You make me feel quite ashamed by your modesty and the equanimity with which you took my — I must call them myself rather rash remarks on Pogonolepis and Thirsophorus, Athrixia and Stoebe, and the old saying is here verified, that the most eminent and cleverest men are the most humble. Those, who know me, know that I am incapable of paying compliments, which I do not mean; but to you I cannot conceal my greatest admiration for your works, in which is united the greatest accuracy with the most implicit veracity, two striking qualities, which are of the highest importance in Natural sciences, the more so, as they are so rare. I gladly confess, that I have learned a great deal from your writings, and that I know few authors, who are so thoroughly master of the subject they write on. I entreat you not to take this as mean flattery but as the expression of my highest esteem and veneration.

You are throughout right, when you say, the great number and diversity of New Holland Compositae should warn us against all unnecessary multiplication of genera. But I might add, that often it is very difficult to decide, which is the type of a new genus, which not, since we have no striking and sure principles in that respect. Most of the older genera being founded on very different principles, different almost in every so natural family. Through many new discoveries the characters of many of them have become most vague and inconstant, and require to be worked up again. — But always, when a generic character is found to be constant, without any transition,

if should be the type of a new genus, moreover, when the habit is sufficiately different; on the other hand, when the character becomes inconstant by new detection, we must not wonder, that another author is invited to reduce it again. Therefore it may be the better, if possible, to adopt or anew to older genera, particularly, if we have not at hand all materials to examine them ourselves. The materials and the literature however increasing in such a degree as in the last thirty years, will render it almost impossible, to dominate the whole, and I fear, that our favourite sciences will be in great confusion in another fifty years, if not every body in time confines himself in one or some of the greater natural families, or in the Flora of his own country. But I will not trouble myself about the future, and successors may continue, what we and our predecessors have begun, – for I said such considerations may lame our own power. I trust that every endeavour to explain the truth, will be a good stone, to the great work, in which we all are occupied.

Supposing that you have not seen the newest Bulletin of the society at Moscou I send you my own copies of Turczaninow's Synantherae, which I received from him. I will inform this gentleman of your wishes, and when you have received them from him, you may return my copies again. I dont expect them earlier. There is however no doubt, that your exposition of New Holland Compositae is the better, and will be adopted in future, and I am sure, that Turczaninow, which I know as a very amiable and modest correspondent, having the most ardent zeal for science, will confess himself, that your definition in general is of more scientifical worth.

I add also Dr. Scaders and Dr. Millers papers on Port Adelaide plants and chiefly Compositae, published in the Linnaea of this year, which Dr Scader gave me for you, begging you to accept them kindly from him. I communicated to him your wishes, to receive a complete set of this interesting collection of Compositae, and although he disposed already of all of them, nevertheless he is willing, to send you the Compositae as complete as possible, but of some of them he has no deplicates; and I am sure by new collections he expecting from Dr. Müller, he will take notice of your wishes in future. I myself have not got myself yet this collection of Compositae he promised to me, but when they are in my hands, and I found anything which can be divided, I will try to complete your set.

I will communicate to Turczaninow your kind offer, on account of Dionaea, and there will no doubt, that he will be much delighted by it. He has a very rich herbarium, and avoid's no expenses to complete it.

Dr Fischer from St. Petersburg, who visited me these days asked me to give you his best compliments, and said you would soon receive a letter from him.

I thank you most sincerely for your little remarks on Turczaninows Compositae as for the four little Compositae enclosed in your letter, which were very welcome to me. I much regret, that my circumstances do not allow me, to make a journey to London. As long as I am physician in our city I am obliged to avoid all expenses, which are not necessary. Some days ago I had the pleasure of seeing Mr. Berthold Seemann, who is gone to the assembly of naturalists and physicians at Tübingen. He too made me long to see the botanical treasures in Kew, but I see no possibility of fulfilling my wishes. He however promised to me, to send me all New-Holland Compositae of Drummond he can procure me from Sir William Hooker.

I should think, that, comparing the descriptions of Turczaninow's Synantherae, you can identify the most of his New Holland Compositae with yours. If so, I beg you kindly to let me know the Synonyms. Dont you know, where are and who has described the plants collected by Galeotti? Some of his Compositae I found determined in your Pontae Wrightianae, but I am not informed if they are worked up elsewhere.

I have delivered the Compositae of Panama for the Botany of the Herald. Amongst them I found an interesting new genus of the Tribe of Eupatoriaceae first Division Alomieae, growing on the aërian roots of the Mangrove tree. You will find it figured. A new species of Chrysanthellum, and another of Unxia also will be figured, but I much regret that a remarkable first new Melanthera with thick subsessile and very small (1/2 – 1 inch) lanceolate leaves and solitary flower-heads on long pedunckles must appear without a drawing. Just now Mr Seemann engaged me for his Compositae and those of Mr Hearne from China, and Dr. Peters Compositae from Mozambique Dr. Klotzsch also gave in my hands. My little paper on Tremandreae and their natural affinity to Rosiopetaleae will leave the press in some days. I shall send you immediately a copy of it. Farewell, my dear friend, and let me hear something from you as soon as possible. With the highest esteem I remain yours truly

Joachim Hecker mpd.

7 Has the U. S. South sea Exploring Expedition also been in China? If so, it would be necessary to compare Seemanns and Hearnes Chinese Compositae with yours, to avoid a new controversy.

Answ. Nov. 24.

Hamburgh, July 8, 1854.

My dear Professor Asa Gray,

On my return from a little journey in Holstein some days ago, I found your kind letter from May, 18, being arrived in the meantime, and I was extremely delighted, though also, I confess, much ashamed by it, considering your most indulgent judgement of my little paper on Panama Compositae on one hand, and my long silence on the other. I received your letter from Nov. 4, and the most acceptable parcel containing the second part of Plantae Wrightianae, and the some of Plantae Lindheimerianae, and the Plantae Texlerianae reached me also in December; but I neglected till now not only to acknowledge the receipt, but also to express my best thanks for your most valuable present. Indeed I beg you most sincerely, to excuse such negligence of mine, being caused by many things, as well in my professional avocation, as in many other scientific occupations or business.

Accept my best thanks my dear friend, for the original specimen of your Pectis filipes, collected by Coulter, whose comparation convinced me, that Dr. Leemanns plant is a different one, being distinguished from yours by the greater number of involucral scales, (6—8) and by the wanting of the elongated aristae in the achaenia. Otherwise it is much resembling your plant in habit, whose pedunckes however I found not ~~at all~~ quite bracteate, but beset about the middle with a very little inconspicuous squamilike bract. I am rectifying my great error in the Addenda to the Flora of the Isthmus in Dr. Leemanns Botany of the Herald. Now I should think, that Dr. Leemanns plant does not differ from Pectis Swartziana, Lessing, whose description agrees in general with the single specimen I examined, and which like Lessings plant differs from all others by the short crownlike but real palaeaceous poppus. Moreover the Flora of Panama abounds in plants, which are indigenous in the West-Indian Islands. Nevertheless it will remain a doubtful plant, because it is wanting of ripe achenia.

I much regret to say, that the collection of Dr. Leemanns Mexican Compositae is not worked up by me, but will be described by the pen of Dr. C. H. Schultz Bipontinus. But the collection of Chinese plants from Hongkong will be determinated hereafter by me. Therefore I am much obliged to you for your kind offer, to send me the sheets as they are printed, when you come in the second volume of your colossal work "the Botany of Wilcke South-Sea Expedition" to the Compositae.

I much admire the fertility of your pen, which has enriched our science with such a great number of valuable works.

Concerning our mutual friend Turczaninow I almost fear, it must have happened something to him. I sent him last December by post direct my little paper on Tremandreae, but I never yet received an answer from him. I feel so much the more, as I know him as one of the most exact of my correspondents. Now he is in the centre of the unfortunate war; but I will write to him and communicate with him your wishes, although I feel not confident, that my letter should reach him safely. However I beg you kindly, to retain my own copies sent to you, because I can peruse myself the copy in the Bulletins of Moscow, which are completes in our public library. Indeed I regret, that my correspondence with Turczaninow and other Russian botanists will be interrupted perhaps for a long time.

On the other hand I ask another favour from you. My first part of plantae Lindheimerianae extends no further than to page 86 till N. 151, (Monarda Lindheimeri), and the second part, for which I am obliged to you, begins with N. 3191 (Clematis Drummondii pag. 1141. I should very happy, if you could complete this most valuable work; otherwise I don't know elsewhere to procure the wanting sheets.

Of my Tuberocheles Rhizophorae I also not possess a specimen. Dr. Leemann communicated with me but very few of his Panama Compositae, but promised me to procure some others of them. As soon as I receive doubles, I shall send them directly to you. I fear however the most of the novelties may be distributed already.

I am much charmed by your kind intention of sending me some interesting Compositae. I am still wanting of many North-American species, even some from New England states, amongst them your interesting Solidago discoidea, and the true Solidago squarrosa Muhlenb. and many others. — So much the more I much deplore, that my friend Dr. Sonder has neglected to send the Australian species of Compositae from Port Adelaide he promised to you, although I often reminded him to do so. I also not received myself these interesting plants from him till now, otherwise I should have divided them with you. Indeed his time is scattered by a good deal of business, but I will not leave off to urge him in fulfilling his promise.

Some days ago I had the pleasure of directing for your a little parcel to Mr. Riding containg five copies of my little pamphlet on the affinity of Tremandreae to Loasopetaleae, and I request as a favour from you to tell your sincere opinion quitely openly, particularly on the subject, I am demonstrating, viz the true affinity of Tremandreae. Will you be so good, my dear friend,

I send you not the reprinting of my Compositae from Panama, because you have the whole Botany of the Herald.

to accept one of the copies kindly from me, and forward the others to their directions? I informed Dr. Riding to reimburse to you on my account all expenses and portages you may have to pay. Two inclosed letters to Dr. Engelmann and Dr. Short also are recommended to your care.

In the parcel directed to yourself, you will find moreover Dr. Fischer's most careful elaborated Synopsis Astragalorum Tragacanthorum, which our mutual friend sent to me in May, some days before my little journey. But very deeply I deplore to have to add the most melancholic notice of the death of this gentleman, whom I valued so much as well as a learned botanist as still more as a true proved friend. I learned this sad news from the papers, and am therefore at a loss to give you any particulars. — Dr. Fischer's work and letter has for us the painful interest, of having been the last scientific production of the author. In an earlier letter from february 14 of this year my friend wrote to me: "We shall be excommunicated by all what proceeds in our science in the West, when that unfortunate war will be broken out, for the communications might be interrupted altogether. I can not think of repeating my journey in Germany; I am most sorry in looking the perspective to see you for a longer time." — When I read this, I did not think, that we should never see us again.

Let me hope, my dear friend, that I can finish my next letter with better news, than with the notice of such lamentable loss of a man, whose memory will remain highly esteemed by us and every botanist, who knew his merits on science and his lovely character. Since a very short time, Botany missed many of its best protectors and illustrious names.

With the greatest regard, my dear friend, I remain.

Yours truly

Joachim Steetz Dr.

I enclose a little fragment of a Composita from Mozambique. The flowerhead however is corroded by insects like all specimens having before me, but I should think, when moistened the anthers and the style of some disk-florets may be found inherent, the ripe achaenia in the fruit-head are well preserved. I should be very obliged to you, if you would have the kindness to tell me your views on it. I cannot bring it to any known genus, and I suppose therefore, that it is a new genus of the Subtribus Helenieae, of which the most genera are of American origin, and well known to you, of course. The branches of the style are gradually attenuated, and the pappus is without any doubt a double one, the chaff of the outer being truncate, of the inner terminating in a long arista. —

I am most delighted, that you are finishing your admirable work: the Flora of North-America. I hope, you will add a Supplement to it.

Hamburgh, february 8th, 1855.

Answ. d. 23.

My dear friend,

I return my best thanks to you for the two kind letters from Novemb. 24th and December 26th, and for some very interesting Memoirs, viz: 1/ Brief characters of some new genera and species of Nyctaginaceae, and of the discovery of two species of Trichomanes, etc. 2/ Characters of some new genera of plants, mostly from Polynesia, etc. 3/ Notes on Cereus gigantens etc. by Dr. Engelmann. 4/ Notices of some new species of Mosses from the pacific Islands etc. by William S. Sullivant, 5/ Remarks on the Monodora, Hl. et B. and Bolivaria, Cham. et Schlechtend. 6/ the characters of two new genera of plants of the order Violaceae, viz Agatea and Rhodendron, and of a new genus of Anonaceae viz. Richella. 7/ Note of the affinities of the genus Vavaea, Benth, and Rhytidandra, A. Gray, — which you had the kindness of sending me, and which were already, with the two fascicles of Tussacianous Synantherae, safely in my hands, sometime before I received your letter from December 26th I suppose, they must be forwarded via England, and I much regret the sorrow, you have made yourself about them. The memoirs destined for my friend Dr. Sonder I directed to him immediately. —

You make indeed too much of my little pamphlet on the affinity of Tremandreae. The judgements about it are very divergent. Some few botanists criticise, that I have pointed out the subject too detailed, whilst others approbate the extensive execution. I think, when I intended to remove an erroneous though in general adopted view, it was necessary, to absolve at once the whole matter, so that a rational objection became quite impossible. The natural affinity of one order to another, depends by no means upon a single homogenous character, but must me resulted by the harmonious appropinquation of all, and by a somewhat adequate habit. — On the other hand the opposition of a single homogenous character, (here the "ovula pendula" in Tremandreae, the "ovula adnexa dentia in Loxiopetaleae,) excludes just as little the mutual affinity of two orders. We observe quite the same relation in Dipsaceae and Compositae, in Borragineae and Labiatae, (in these moreover an other opposition, viz leaves alternates in the former, leaves opposites in the regard other order,) in Laurineae and Atherospermeae, in Daphnoideae and Elaeagneae, in Restiaceae and Cyperaceae, etc. etc.; and no one indeed, I should think, can oppose anything against the mutual affinity of each of these combined orders! —

I much regret, that my medical and obstetrical avocation prevents me so often, to spend more time on botanical studies, and by the same cause I am hindered,

leave my residence. Therefore I cannot hope, to visit England in the next time, and to make there a collation of Turczaninows and Gouans Compositae in Sir Hookers herbarium.

I thank you for the nice Pericome candata, enclosed in your letter from Nov. 24th I also received your Bertis filipes, which you had the kindness to send me sometime ago. Both came well preserved into my hands. My copy of Plantae Lindheimerianae is complete indeed! I was deceived by the N: ** 151 on page 56, whilst I overlooked the N: 318 on page 55. I thank you most heartely for your wish and for your willingness of completing me this valuable work. Dr. Sonder has not forgotten your desire, concerning Dr. Müllers Australian Compositae, but hitherto he found no time, to prepare a parcel for you. The new collection expected by him, is not arrived till now. A large collection in the last year has been lost in the Indian Ocean.

In N: 2 of the Bonplandia from February 1th, 1855 you will find a Necrolog of our lamented friend, Dr. Fischer, from mypen. It is said there, that his excellent Herbarium will be sent to Hamburgh. But I am informed to day, by his widow, that it will remain for a while in St. Petersbourgh, because the Russian government intends to buy it. I believe, that you have seen it in St. Petersbourgh, and that you know the high scientific value of it. Therefore I am authorized by the widow, to consult you, if she can hope of selling it in the United States. Would it not be desirable, when the Smithsonian institution could acquire it? She proposes a prize of 5000 Dollars for the whole herbarium inclusive the valuable and large collections of woods and seeds. It contains certainly the most complete collection of the extensive Russian Flora, and is said to be in a very good condition. — I beg you, my dear friend, to answer me immediately, if you can hope — who knows best the circumstances in your own country — to procure her such a prize? If not, she is forced, to leave it to the Russian government for a lower prize. — Therefore I would much obliged to you, if you could inform me as soon as possible about this subject. I am sure, you are willing to do such a favour to the widow of our mutual lamented friend, and you certainly will be happy with me, if you can do anything to soften her sorrowful situation, in which she suddenly fell by the unexpected and mournful death

Excuse my bad English style and my blunders, but I am in the greatest hurry. I am, my dear friend with the highest esteem for ever

Yours very respectfully
Joachim Reetz D.

pr. Steamer, via England.

Professor Dr. Asa Gray

in the Harvard University

Cambridge

near Boston

Frauco

Hamburgh, August the 30th 1855.

My dear friend,

I am ashamed indeed, that I have not answered earlier your kind two letters, from March, the 23th and June, the 15th, so much the more as the latter was accompanied by a most welcome present, the likeness of yourself. I am much delighted in looking upon it, and in divining from it your good temper, your scientific zeal and earnest, your ingenious ability in well observing and distinguishing everything. Surely, a good likeness is a charming surrogate for want of personal acquaintance; and I give you my best thanks for it. I took, as good advised me, the grayish one, not mounted at the card-board, for myself, and sent the other brownish coloured to Dr. C. H. Schultz, Bip. But mine is now mounted likewise and well preserved in a nice frame. You may presume the great value, which I am putting upon it, when I say, that you will find in my next reading for one likeness, thirteen others in return, amongst them my own. For I don't possess other copies of a likeness from me, besides a greater lithograph, representing an assembly of thirteen physicians of Hamburgh, which are associated to a scientific Society, whose meetings are held every friday's evening. When this Society, some years ago, celebrated their Jubilee of twenty five years, we sat for a Daguerrotyp altogether, from which the lithograph is sketched. I will add the names of every one of them, because it could be, that one's or other's reputation is known to you, and beg you, my dear friend, to do us the honour, to accept it kindly.

On the other hand, I am much obliged to you for two very interesting pamphlets, you had again the kindness of adressing to me, viz the "Plantae novae Thurberianae," and your "note on the affinities of Vaccea, Beath. and Rhytidandra", A. G. of which latter however I formerly received a copy from your goodness. The copies for Dr. Sonder and Mr. Booth I forwarded to them, and the other two, not marked, I suppose you have destined for our "Naturwissenschaftlichen Verein."

I will add to the lithograph a few plants, received some month's ago from Labrador. Amongst them there are some, which in your excellent Flora of North-

America are not enumerated, viz Erigeron pulchellus, Blaalaschkeare, Cod, the true species, which seems different to me from E. naiflorus, Lin. Erigeron drae. bachearis, Hook., certainly the same as E. elongatus, Ledeb. but this is a pretty variety, with the stem and leaves very hirsute. The true Antennaria monocephala, Cod, which certainly differs from Antennaria alpina Gaertn. with solitary heads, having the involucral scales much more blunt, Gnaphalium Norvegicum, Gunn., Phaca oroboides, Cod. Arenaria Norvegica, Gunn., var. frigidissima, Fries, a little rare plant, Cerastium alpinum Lin. Draba arabisans Michx., Ranunculus affinis, R.Br. or a new species, and others not mentioned as occurring in Labrador, also the rare Eriophorum Russeolum, Fries, etc.

Moreover I thank you for your answer belonging to the Herbarium of our lamented mutual friend, Dr. Fischer, whose 9 wanting tables to his "Synopsis Astragalorum Tragacantharum also will be enclosed into my next reading. Your answer came to me in time, so that the widow was enabled to transact with the Russian Government, and I believe, that it has acquired this valuable Herbarium, but I fear for a very small price. She wrote to me, that she was willing now, to sell it at any price, but afterwards I heard nothing more of it. I must say, that I fancied, that it was not in the plan of the Smithsonian Institution to make acquisition of foreign plants, but I would not neglect, to make the trial.

Now, my dear friend, I have a new request. One of our public libraries belonging to the Commercium (Board of Trade) has already of the work of Capt. Wilcke's expedition what appeared till now, but is wanting still of your first Volume of the splendid Botany belonging to it. The library received its parcel from England, but to a higher price as it is sold in the United States. I am authorized by the Director of the library, to entreat you, that you may have the goodness, to send to me the Botany as far as it appeared with the Atlas of 100 plates, and to procure it to a price so moderate as possible. I beg you however to add the account of it, and to let me know, to whom it can be paid here. Then the reimbursement immediately will be returned. — In an earlier letter you had the kindness of promising me, to send every sheet as soon as it appeared, when you will come to the Ordre of Compositae. I should be much delighted, if you would kindly remember this most welcome offer.

You noticed in your letter, that Mr. Ploechow takes the same view about the affinity of Frenandrea as I myself. I presume you mean the little Note in the "Annales des sciences naturelles, 4me Serie première année, T. 2 No 5. (1854) pag 265 sub Aristotelia, Herit." Or has Ploechow in an other place, which might have escaped my notice, adopted my views? Has he perhaps criticised the little pamphlet, and where? Tell me that, I beg you, in your next letter.

I see, that the "Bonplandia" comes into your hands. Then you have perhaps read my little treatises versus Dr. Lindley. I much regret, that Dr. Seemann engaged me, against my wishes in this controversy, which was followed by some injurious anonymous notes, subscribed with "X." and: "Ein deutscher Botaniker." — by publishing my first private letter. It is most unfair to abuse the anonymity in such a manner, and my numerous friends, and Dr. Seemann himself are exasperated by it. The mean author however is detected already, as I never was in doubt of him. — Let me add, that he is an ignorant throughout, that he has no idea of science and scientific observation, and that all, what is published by him, is either a nonsense or blunder, or elaborated by others. With such men and such arms I am quite unable to fight. [illegible] in regard of the "antherae of Waitzia" I hope to compose the difference with my friend Dr. Sonder, which is an opponent most respectable. The anonymous, as you can see, publishes the half of my note and Dr. Sonder's, the other half of both he prefers to conceal, for his aim is not to clear up science, but to offend and to spoil my reputation. — Dr. Seemann should suppress such squibbles, being of no interest for science, and indeed his journal has lost, by its inexperienced redaction, the sympathy of most learned botanists. I told him that openly, and I hope he will become more circumspect. Otherwise, I am sure, he will provoke every moment.

I repeat my apprecations, that you may excuse my retarded answer, being hindered by a troublesome practice and by many other things. Let me hope, that you will not do the same.

With highest regard and esteem I remain

my dear friend

Yours most faithfully
Joachim Steetz Dr.

Will you have the kindness to forward the enclosed letter of Dr. Sonder to his address?

Nach Postzeit

BOSTON SEP 20 AM PAID

p. Steamer via England

HAMBURG
31 AUG 1855

5

Professor Dr. Asa Gray

Paid

Cambridge
near Boston
(United States)

Hamburgh, the 29th May, 1856.

My dear friend,

You will wonder at my long silence, and be fully justified to blame me for it, and form a bad idea of my passion for correspondence. Indeed I confess, that your immediate answer after being returned from your voyage to Paris and London and by the great pressure of your time, had deserved greater and better acknowledgment from my part. But that is the bad consequence of the delay, that it increases every day, in almost geometrical progression. Therefore, as I am unable to defend myself in another manner, I beg you, to forgive me. —

On the other hand, it is true that my time is dispersed by many things, which do not belong to my very professional business. I am corresponding Secretary in our Society of Natural Society, of our Horticultural Society, and of the surveying Committee of our National Museum for Natural history, and all these offices require the greatest deal of my leasure time, by numerous sessions, Protocol-writings and a great correspondence, whilst my medical practice demands the best time of the day, so that but a very little rest of it remains for Botany.

I am very glad, to read in your letter, that the anxiety, you had at your setting out, was quite removed, when you arrived at Paris, finding your brother-in law entirely convalescent. At the same time almost, when you were in London, I was in Glasgow, following the kind invitation to the meeting of the British Association for the advancement of science, where Mr. Ward of London, with whom I met with Mr. Gourlie, (a Zealous Botanist of Glasgow,) informed me of your being in Europe, but he did not know, in what part of our Continent. I much regretted, that my professional relations did not allow a longer absence from home, so that I might have prolonged my excursion to London, where I would have had the high pleasure of perhaps meeting with you. You know, that neither Sir William Hooker, nor Dr. Hooker, nor Bentham, nor Lindley etc. visited at that time Glasgow, as I hoped. Besides Mr. Gourlie, Dr. Balfour, Mr. Ward, Prof. Balfour, Prof. Dickie from Aberdeen, Prof. ~~Fleming~~ Fleming, etc. there were no other botanists of some reputation, of whom I had the pleasure to make their personal acquaintance. Nevertheless I was most satisfied with this little excursion, by which I am indebted for many interesting connexion with other learned men, and for a great deal of hospitality and real British courtesy.

I hope, you will have received already my little parcel, I directed to you through Mr. Röding, about two months ago. A few days, after having forwarded it, I was attacked by a violent gastric fever, which kept me in bed full eight days,

and a fortnight moreover at home. Then I began my medical practice, but in the first week with the feeling of great debility. At present I am quite recovered and I feel enabled to take up all my usual exertions.

In regard of your Botany of ~~Capt. Wilkes~~ the U. St. exploring expedition under Capt. Wilkes I am most obliged for your readiness in fulfilling my wishes, and I beg you to send it to me as soon as the Atlas will be appeared. But the Director of our public Library entreats of you, to add your bill of costs, including the premium for insurance, which will be immediately reimbursed to the hands of Mr. Rödiger. Moreover I beg you, to add the cryptogamical part, which mean-while is published by Mr. Brackenridge, and if possible, all other publications concerning Natural history and supplying Capt. Wilkes expedition, of which our Library has no more than the 5 Volumes and the Atlas of the "Narrative of the U. St. exploring expedition during the years 1838–1842," which appeared in 1845 etc. If it is not too late, to send it in the annual envoy of the Smithsonian Institution, this way would be the most preferable, otherwise I beg you, to forward it by first other ship starting for Hamburgh, and to take the insurance for the full value.

Have you taken notice of Planchon's remark in the Flore des Serres accompanying the drawing of Tetratheca ericoides? I have not seen a specimen of Crinodendron, but I have inquired the figures of Cavanilles and Hooker, and I must say, that I am by now means convinced by his view, that Crinodendron should have a nearer affinity to Tremandreae than the Order of Loriopetalae. Besides that, the general view concerning Elaeocarpeae is much earlier pronounced by Reichenbach, sen., viz. 1827, as I have referred in my little paper, pag. 40, whilst I endeavoured pag. 48 and 49 to point out the arguments, by which I was induced to take a closer affinity between Tremandreae and Loriopetalae, than Elaeocarpeae, and Reichenbach himself confirmed my view, in a very kind letter, in which he abolished his own opinion.

I am glad to tell you, that you will soon receive a letter and probably also a great deal of Dr. Müller's South-Australian Compositae from my friend Dr. Lindley, as he told me some days ago. He also received a fine collection of plants from Texas, and I suppose, that he will ask your opinion for some of them.

I direct your attention to a little paper: "Die Befruchtung der Phanerogamen. Ein Beitrag zur Entscheidung des darüber bestehenden Streites. Inauguraldissertation. Mit 3 lithographirten Tafeln. Leipzig bei Engelmann. 1856. 4to." published by a young German botanist, Dr. Ludwig Radlkofer of Munich, who visited me just now, some days before I became ill, and who showed me his fine preparates. He is a very intelligent and modest young man, and has brought the controverse of the Embryogenie in a new phasis. He has demonstrated by a great number of microscopical researches, made in many plants, but particularly in Euphrasia Odontites, L. that the utriculus of the Pollen, (germanice: der Pollenschlauch,) never comes in contact with the sacculus embryonalis. Dr. Radlkofer found by the section of the ovulum, before fecundation, that there are always at the end of the sacculus embryonalis, which is situated nearest the Micropyle, two little germinating cells, (germanice: Keimbläschen,) of unequal size and situation, the greater of which, occupating the very apex, where after fecundation the utriculus of the pollen leans against, whilst the smaller one, situated sidewards never reaches the apex. Dr. Radlkofer furthermore observed, that the greater ~~germ~~ one always obliterates, whilst the smaller, which is entirely separated from the utriculus of the pollen by the greater "Keimbläschen," and therefore never comes in contact with it, after fecundation swells, and thus becomes the real bearer of embryo. (I hope, you will understand my brief explication in general.) His great collection of accurate Präparates have convinced even my friend Professor Schleiden at Jena, who partly gave up his own theory. Dr. Radlkofer is gone from here to London, and probably you have already heard more particulars of this new and interesting discovery.

Believe me to remain, my dear friend, with the highest esteem

Yours very faithfully and truly

Joachim Steetz.

I send this letter with the first Steamer starting directly from Hamburgh to New York. She is the "Borussia," which with the "Hansonia," of our "Hamburgh" Steam Navigation Company will entertain a monthly direct communication with the United States, from and our town.

If the price of them does not exceed the sum of about fifty Dollars, otherwise I beg you to inform me previously before sending them.

p. Borussia.

Med. Dr. Asa Gray

Professor of Botany in Harvard

University, &c. &c.

Cambridge

near Boston U. St.

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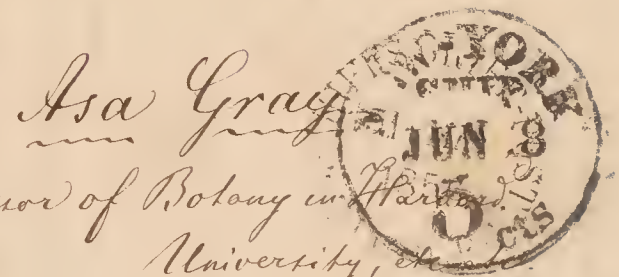

Hamburgh, July 16th. 1856.

My dear Professor Asa Gray,

I have the pleasure of introducing to you my friend Dr. Riesser of Hamburgh, who is upon the point of leaving our continent for a visit into your beautiful country.

Dr. Riesser is neither physician nor botanist, he is lawyer, and a learned man in the fullest signification of the word, and has the utmost interest to become acquainted with the institutions of your celebrated Harvard University.

Therefore I shall be very obliged to you, if you will have the kindness to assist him in attaining his aims. On the other hand I hope, that the personal acquaintance of my friend will be for you of no little interest, and I should be very happy, when you would give me an opportunity, to return your services in a similar way.

I am, my dear friend, with the highest

esteem

Yours

very truly

Joachim Raff

Hamburgh, April 20th 1857.

My dear Professor Asa Gray,

It should seem indeed, that I am a very negligent correspondent and a very ungrateful one too, as I have before me two kind letters from you from July 30th and Novemb. 17th, 1856 unanswered, which I had the great pleasure of receiving many month's ago. — But I beg you sincerely to excuse my long delay, which is caused, as you know, by the want of time, being scattered by my medical practice, so that very little of it remains for Botany. Believe me, my dear friend, it is far from my own interest, that I am pressed to do so, than I can say, that every day bringing me a letter of your hand writing is regarded by me as a holy day, having the highest influence upon my state of mind and humour. And therefore I risk not to answer in a hasting disposition, and waiting for a quiet moment, I forget, that such is a rarity by so many occasions in various manner. Let me add, that a little obstacle also is the foreign language, to which, (it is singular to say, but true) I am inclined not every day. If I am distressed by business no sound idea can be brought to paper in a foreign language.

My friend, Dr. Riepen is returned safe and well and quite satisfied with his voyage. He has brought me your compliment, and is charmed by the kindness of so many distinguished men, to whose he felt obliged during his sejourn in America. I thank you heartily for his kind reception.

I am very glad to hear, that my small parcel of Labrador plants was welcome to you. I feared indeed, that it contained nothing new for you. Pray, is the Ranunculus amongst them the true R. affinis, R. Br. or a new species?

Asia, viz Japan, particularly in account of so many genera, identical in both, which are wanting in Europe.

I must come back to my elaboration of Mozambique Compositae, which progresses with great delay, because I found an erroneous observation, made by Lessing and de Candolle, and by which I was inducted to stop the work for a time, and to reexamine a great deal of known species. As I think, that you are now in working up the Compositae of Capt. Wilkes's Expedition, I fancy, that it could be of some interest for you, when I tell you, what I found, and therefore I beg your pardon, if my letter should increase in a manner, which could fatigue your patience.

Lessing and also de Candolle state, that the Pollen in the whole Tribus of Vernoniaceae is globose and echinulate. By this fact I became dubious, if a new genus, belonging as I supposed to this tribe, should be indeed a true Vernoniacea. The Corolla, the style, the anthers, the achaenia and the whole habit were that of a true Vernoniacea. Then I examined the pollen, by a magnification of 15, and I was struck to find it even. Should it be a Mutisiacea? I examined the pollen of Moscharia, it was also even but in an other kind. Bringing the two pollen granules under my Microscope with a magnification of 200, I perceived in a moment the great difference between both. The pollen of Moscharia remained so magnified even at all, and was not globose but a little oblong. The pollen of my new genus was now not even, but a little granulate, but not echinulate, and was not globose, but polyëdrous, as we find it in many Cichoraceae, for instance in Taraxacum vulgare. Therefore my new genus could not belong to the tribe of Mutisiaceae, and was in deed by all other characters a true Vernoniacea. But after having examined a great deal of known Vernoniaceae and having found in all a pollen globose and echinulate, new doubts arose. I was in deed desperate, and in the point of giving up new reexaminations. Some days were passed and considering the difficulty, because I began to have no true confidence in my own observation, but then,

With great regret I perceived the consumtion by a great fire in Philadelphia of Brackenridge's Folioes of Capt. Wilcke's Exploring expedition, particularly as you fear, that it will not be reprinted for some time. It is highly interesting, that by good hap some Copies and the copper-plates were saved.

I anxiously expect your New Manual of the Botany of the United States, which you had the kindness of destining home, and the first Volume of the Botany of Capt. Wilcke's Exploring expedition. Both however I received not yet, but I hope, that they will be forwarded soon from Leipzig, where the packages of the Smithsonian Institution commonly are directed. My best thanks for such a valuable present.

With great pleasure I have read your notice on Radlkofers paper, who a new paper published showing the relations of the Process of fructification in the Vegetable Kingdom to the animal reign, being almost identical in both. Radlkofer indeed is an accurate observer, and being a very young man of independent circumstances, science can expect many valuable contributions from him, the much the more, as he is a zealous and ardent adorer, of our "scientia amabilis".

Still more interested me your Statistics of the Flora of the Northern United States, of which I have read two sections, the last in No 67, January, 1847. The comparation of your Flora of that with Europe gives results of great importance, and invites urgently to a more accurate examination, as hitherto is done, of the allied species in both Flora's. But for this purpose it is not enough, to compare a single or a few specimens of each Continent, but a great series from most different localities of each species must be had. Then, but not earlier will be settled the great Controverse of our day: what is good species, what is a good genus. Till now nothing is stated, than the fact, that the various orders of plants are worked up in an unequal manner, and by different principles, so that many of them need a new and more accurate elaboration. And on the other hand the fact is stated, that all orders, or at least most of them with a multiplication of genera since Linnaeus, are not suffering under such a confusion how others. Linnaeus distinguished with a great sagacity genera in such orders, which were numerously represented in his own country, as in the Leguminosae, Rosaceae, Compositae and others, and thus he was conducted confidently often more by considering in a sound and happy manner the habit, than an essential and strict character, otherwise he would not have distinguished, Lathyrus, Orobus and Vicia, Hieracium and Crepis. But he was forced in the mean time to combine exotic species of different genera in a single one, by a want of materials, so that the consequence was an unequal elaboration of genera in general. Nevertheless his system in many instances is a mere prototype of a sound one, the much the more, as in the last years of his eminent and productive life he gave the first lines of a real natural system, feeling himself the want of a natural arrangement in his Sexual one. Now it is an other matter. Whilst the exotic plants, almost from every country, and the new detections have increased to such a great number, it is necessary to distinguish them in the same careful manner than Linnaeus did in founding genera and species of his own Country. Therefore I consent wholly with you in your notice to Dr. Schultz's Rep. elaboration of the Mexican Compositae in Leemann's Botany of the Herald, which I found in Silliman's Journal. I can not approbate the reduction of so many good and natural genera. Our friend seems to love extremes. And working up the Compositae of Dr. Peters from Mozambique I am in the point of restituting many of his own genera, which afterwards were reduced by himself. And I hope to show that they are good genera, when the character is emendated.

I anxiously expect however the continuation of your Statistics, and I should think, that you will point out a greater affiance of your Flora to that of the North-east of Asia,

time, and therefore I putted aside all other business being not pressed, and the letter was not sent.

Now a fortnight ago I received your very welcome package, containing your wished anxiously expected Botany of Capt. Wilcke's Exploring Expedition for our (public) Library, the second edition of your excellent Manual of the Botany, your Statistics of the Flora of the Northern United States, first article, and Dr Engelmann's Synopsis of United States Cactaceae, and I say you my best thanks for such a valuable present. With the greatest pleasure I have threw a glance on your Manual, and I admire your rare skill of making short and precise diagnoses, in which the characteristical property of every plant with few words is laid down. No other than you, who knows so well the Flora of your own country can write such an useful book not only for the early but also for every advanced Botanist, who is on the point of being acquainted with your rich and interesting Flora.

While more I was struck by your eminent erudition and your immense knowledge of plants, by which only you were enabled to work up the Flora's of such quite unknown Islands, viz: the Feejee, the Navigators, Tonga, Society, Friendly and many other Islands, which abound, like almost every isolated Islands Flora, as is known, of a so great number of peculiar plants and paradox vegetation, different from that of every other country. and on the other hand I admire mostly your knowledge of the Flora's of so many other districts, which were visited by Capt. Wilcke, and of which you recognized so many known species. Indeed that is a splendid work, giving you the very claim of immortality in the Annals of Botany. I anxiously expect your Atlas too, and beg you, to send it, as soon as it appears, with one of our Hamburgh Steamers, accompanied with your bill of costs, which immediately will be reimbursed to Mr. Rödiger for your account.

Also I beg you, my dear friend, to tell me in your next answer what are the costs of: 1) Dana, Report on the Crustacea of the exploring expedition under Capt. Wilcke. 1852. 2 Vol. and 2) Dana, Structure and classification of Zoophytes. Philadelphia 1846. 4. United States Exploring Expedition under Capt. Wilkes Vol VII. Tell me also, if you will have the goodness to procure and to send (it to) them to me, if it should afterwards be wanted by our Commercial Library, which however would give me no certain resolution, before it knows the price of these works. — I have read the conclusion of your mostly interesting Statistics in the May number of Silliman's Journal. Nevertheless I would be very thankful to you, if you would have the kindness to sent me also the continuations of your first article, because it is a very useful and valuable Contribution for Botanical Geography.

2. weighing considering the habit of my new genus, I inquired for an alliance of it* anew, and thus I was conducted to reexamine the pollen of some Indian Vernoniaceae, being described by de Candolle himself in his Prodromus. The first of these was Decaneurum reticulatum, DC. and I was much rejoiced to find a similar pollen. The same result in D. molle, DC. Now I was sure, my genus belonged to the Tribe of Vernoniaceae. I will not fatigue you with particularities, but tell only the fact, that I found the same in more than twenty long known Vernoniaceae, especially in Asiatic and African Vernoniaceae; and also in an American one, in Checkia cyanea, Elliott, in which I suspected it also indeed, by its foreign habit. Considering, that I found in Gymnanthemum fimbrilliferum, and cupulare, Cassini a pollen globorum echinulatum, which both are reduced by de Candolle to his Decaneurum, I perceived at once the great value of this fact for classification.

On the other hand I will not wonder, if it should be considered by many botanists as a trifle, and that they should ejaculate a loud cry, when I require, that in future the Pollen must be examined in every supposed species, and that of it must be taken notice in the same manner, as of all other organs of the flower and fruit, when Systematic Botany indeed shall be purified from errors. Nevertheless that is a request as a conditio sine qua non. And it is a request, not first asked by myself, but it is an old one, asked by R. Brown, Cassini, Lessing and de Candolle, who inquired also in the pollen, and added it to the essential characters, on which they founded their great Tribes in the vast and difficult Order of Compositae; and to whom we are obliged to an most ingenious and classification of this Order. But the fact is, that the golden principle of our learned and eminent predecessors seem to be forgotten by many modern Botanists, viz. to examine accurately every part of the plant, and therefore it is the very time to renew such request, if the whole Systematic Botany shall not perish in such a confusion, as has begun since the last twenty years. I don't hesitate to confess, that the Vernoniacea are one of the weakest Tribe of de Candolle's elaboration of the Compositae, because de Candolle, beginning his great work had not the surety of weighing the value of different characters, which he procured himself by its progress.

Nevertheless his elaboration even of the Vernoniaceae is the best, we have at time, and therefore indeed, it would be dangerous to shake on its fundaments. On the other hand it is not necessary to do so, viz to alter the scientific principles, but only to purify the Tribe from the errors, which slipped in by a want of consequence, adopted in the elaboration of the later Tribes. To this respect I judge: 1/ that each Order of a great extent needs particularly a careful and minute distinction, because the individuals in general have a greater resemblance and a nearer alliance with one another, than in other Orders of a little extent. 2/ that the fact, that so many vast genera in Compositae, as Vernonia, Senecio, etc. remained next to many others, consisting of no more than one or a few species, is not always the result of a natural necessity, but the necessary consequence of an unequal elaboration of the whole order, of the want of careful examination and of strict principles in the way for settling genera and species, which however scarcely can be fixed in general, but must be left adopted for every single Order especially, because the value of every character seems to be different in different Orders. 3/ that constant characters can and must be found out, by which such vast genera can be divided in some new and well distinguishable genera, as already was done by de Candolle and others, in the original genera: Aster, dividing Gnaphalium Conyza etc. 4/ that in this respect notice must be taken of the Geographical distribution of individuals, because the most natural genera often are confined to very small Geographical range 5/ that in each Order, in which these requests are already executed in a natural manner, the recognition of plants is more facilitated, than in others, in which vast genera still abound 6/ that it is therefore the duty of every one, to clear up errors or inconsequences in this respect, when he has the opportunity to do so; but than not in an aphoristical, but in as to full, extensive and detailed manner as possible.

So I hope to work up the Compositae of Mozambique, to emend some characters of known genera, and to define some new ones. On the other hand I much regret, that the object of the whole work, of which my little elaboration is only a part and the time resting me for it, allows me not, to work up a revision of the whole Tribe of Vernoniaceae, so much the more, as I was unable to procure me all necessary materials at hand. But I must confess, that the inquiry of the pollen alone, has spread many light over the value of genera. There is surety and good and clear reason, that two species cannot belong to one genus, of which one has the pollen globose and echinulate, the other polyëdrous and granulate. The pollen being so constant as few other organs, and showing so little differences in the great Order of Compositae, that it is of the same appearance in many thousand species, is not only a good generical character, but by a good logic it should be required for a Division of a Subtribe, but not as a character for a Tribe, because the genera with a polyëdrous pollen can not be separated by any other character from the other genera of Vernoniaceae, with having the pollen globose and echinulate. Moreover I found some other characters mostly constant in Vernoniaceae, viz. the way base of the style, and its articulation with the achaenium through that intermediate organ, which is named the Nectarium by Lessing; also the shape of the Nectarium itself is very constant and significant. And when all these characters, proved to be constant by Lessing, Cassini and de Candolle, and therefore adopted for the distinction of many genera by them, are to be neglected in by others, and overlooked indeed by many modern Botanists at all, a good Classification of Compositae will remain for a long time a "pium desiderium". And certainly, it is quite impossible to distinguish with certainty the species of these vast genera, being over-loaded at time by so great number of Types of different and good genera.

I would be very happy, if so an accurate observer, as you are, my dear friend, to whom Botany is indebted for so many and so real progresses, could ascertain my fact, and could make any use of it, in working up the Compositae of Capt. Wilkes' Exploring expedition, and therefore, and because you are a much more productive author than myself, I am far from making a secret of it. My chief point is progress of science, by whom it is made, is quite indifferent.

July 28th. 1857.

When this was written to be forwarded with the first May Steamer, I was interrupted to do so by my friend Dr. Seemann who asked me to work up his Compositae from Hongkong for the Botany of the Herald. This however must be done in a very short

3. I am now on the point of finishing my Vernoniaceae of Mozambique, being interrupted almost two months. But I hope to communicate with you in my next letter a Conspectus of the genera, which are treated there. To any restored genera of Schultz Bip., viz: Gutenbergia, Shengelia, Linzda (which however are to be furnished with an emendate character,) I must add a few new ones. In this point, I am unable at time to give a strict character of the old genus Vernonia of Schreber, but now reduced by myself, because the space permitted to me was too confined, as that I could bring into discussion also the American, and also the whole of Asiatic even African species. I regret this so much the more, as probably Schrebers Vernonia, (though none species of it is described by himself,) was founded on North-American or West-Indian species. Therefore most of these must remain in Vernonia, and I concluded previously, to leave in Vernonia all species, which show the most striking characters, given by Schreber, and others, accepted by de Candolle in his prodromus. Viz: Pappus biserialis, exterior brevissimus paleaceus. (Schreb.) Corollae lobi parte integra subaequales. Flores purpurei, rosei aut albi, nunquam lutei. Pollen globoseum echinulatum (de Cand.) To these I added: Stylus basi incrassatus, by which it is distinguished characteristically enough from the allied, but truly different genus Gymnanthemum, Cass., having the style attenuate at the base. A strict and careful character of Vernonia cannot be given, before all species, hitherto referred to it are accurately reexamined. That de Candolle was not clear what he should bring to Vernonia, what to his Decaneurum, he has shown by the fact, that some good and true species belonging to Gymnanthemum, Cassini, are brought by him to Vernonia, and that no more than 4 species, (or perhaps viz, D. grande and Decaneurum globrum, DC. being unknown to me) of his second section of Decaneurum, really belong to Gymnanthemum, Cass., of which one ~~and the same~~ species, viz - Gymnanthemum fimbrilliferum, Cass. is ~~twice~~ enumerated twice, as Vernonia fimbrilliferа, Less. p. 23 n. 49. and as Decan. fimbrilliferum, DC. p. 68. n. 9. - De Candolle's enumeration of Eupatorium is a much more natural one indeed: but when genera, as Decachaeta, Oclinium, Conoclinium, Hebeclinium, Crumpitzia, Clinium and Critonia are removed by him from Eupatorium, and perhaps with good reason, many other of his genuine species of Eupatorium must be excluded by the same principle, and therefore by a good logic also. So for instance, if I am not wrong, belongs Eupatorium foliolosum, of which I have seen an original specimen gathered by Gaudichaud, to the good and characteristical genus Nothites of Cassini, which however seems to have been unknown to him. I have seen in the Royal herbarium at Berlin some specimens of Nothites, gathered by Commerson at Montevideo, but wrong-named, and without any name. They belong certainly to Nothites. To on the other hand most species of de Candolle's § 1 Cylindrocephala have a conic receptacle, and cannot therefore remain in Eupatorium, for which de Candolle decided a flat receptacle.

I hope my dear friend, you will not misunderstand me. I will not criticize, but only demonstrate, that improvement is necessary. I am indeed far from making the slightest reproach to our great master, for the result of his great work: the Classification of Compositae in his Prodromus. At time we have no better, and it was quite impossible, to subject each species to such an accurate examination, that he could perceive this its whole all, once to, as well its whole difference from all others, if he would bring to an end at all, this immense work, the total elaboration of the vastest and difficultest Order of the whole vegetable Kingdom. It may be added to de Candolle's very apology, that he had not the opportunity of seeing all species enumerated by him, viz: all species gathered by Humboldt, described by Kunth, all species, described by Hesting and many others, which were accepted only on the authority of their authors. and still more: Botonical science is really indebted to him for his classification being a true improvement, founded on a firm ground, and there is nothing or very little to alter, having already been adopted the best and constantest principles by himself. — Therefore all successors of him have not half the labour after him, because the path is beaten and well marked by good leaders, and we have nothing to do, as to plain the way, and to accomodate new and necessary contributions from time to time. If anything deserves reproach, so is it the fact, that some modern authors have lost the wright way, and taking bad courses they have increased confusion in descriptive and Systematic Botany. We need no other improvement than such, which arises or proceeds in applying de Candolle's own principles in a somewhat more accurate manner, as was possible in the first general application of them.

But it is more than time indeed to conclude my letter, being increased reluctantly to so a great space, having already laid claim too much on your patience. Therefore I beg you sincerely to excuse me, and to let me hope, that you will maintain also in future your favour so highly estimated by me.

I am, my dear friend, with greatest regard

Yours truly

Joachim Steetzius

Hamburgh, Novemb. 30th 1857.

My dear Professor Asa Gray,

Some days ago I had the great pleasure of receiving through the Smithsonian Institution the two packages kindly directed from you to me, containing your splendid Atlas of the Botany of the U. St. Exploring expedition for our public Library, and moreover: The Botany of Beckwith's expedition with 10 plates, the Botany of Pope's expedition, (without plates, which I should be glad to receive still) and two fascicles of your interesting Statistics of the Flora of the Northern United States.

Your friendly letter advising to me these most welcome treasures, and containing the bill of costs, reached me at the end of August, just when I had addressed to you scarcely three weeks, before a long letter. Therefore after having reimbursed to Mr. Röding for you 30 American Dollars for the letterpress, (previous received) and the Atlas of the Botany of the U. St. Exploring expedition, I stopped my answer, till I could at once inform you of the safe arrival of the packages. Now I do so hastely, the much the more, as since I had anew the pleasure of receiving from you through my friend Dr. Seemann your interesting List of dried plants collected in Japan by Williams and Morrow.

Allow me, my dear friend, to say you my best thanks for such a valuable contribution to my library. I am ~~+~~ admire your activity, with which you have done in so short a time so much for the advancement

of our science. That is no simple compliment, but the very truth! I confess, your conscious and accurate method of examining and describing plants was a model for me since a long time, and I believe, that there are but few of our contemporaries who are acquainted so familiar with so an immense number of plants, than you. Therefore our science is indebted to you for innumerous improvements, as you have cleared up so many errors of the others, settled so many Synonymy, added so many new and real detections, finding out their right situation in the Syst natural System and publishing their careful descriptions, notwithstanding the many splendid drawings, which we owe to the skilful pencil of Mrs. Sprague under your scientific assistance and survey, which surpass by far, considering their precise and elegant execution, those of Mrs. Fitch, who however, as well as Mrs. Sprague, is also very prosperous in hitting the habit of the plants. Because the Atlas was not bound, it is immediately sent to the bookbinder, so that I have till now only caught a glance of it. But I was struck by the immense number of new genera and of others, quite unknown to me. There seems to be a very peculiar vegetation in the Feejee and other adjacent islands.

I anxiously expect your answer of my long and perhaps too prolix letter; but the matter treated of therein, is of too great an interest for systematic Botany, that, fearing to fail a clear definition of that, what I would say, (particularly as I wrote in a language (being not vernacular to me,) I avoided not prolixity. I have read in Silliman's Journal your views about Beatham's Memorandum on the Principles of generic nomenclature in No 4 of Vol. 1 of the Journal of the proceedings of the Linnean Society, and I believe to have understood, that you not fully consent with it, like myself. I find not that clearness, which requires the exposition of such a difficult and important matter, and which we met with in other works of that celebrated and also by me highly estimated author.

I was much interested to hear many particulars about you by my friend Seemann, who had the enviable opportunity of living some days with you.

I add my little paper on Hongkong Compositae in Seemann's Botany of the Herald, and an article on Dahalea Chinensis, DC. in the Bonplandia of Nov. 1, 1857., both recommended to your indulgent judgement. I much regret that I have no more to send, and that thus my debts owing to your friendship increase every year.

I am, my dear friend with highest esteem

Yours very truly
Joachim Steetz

Excuse, my dear friend the forwardness of my request. Truth in the cause of Natural science is the first requisite, and for me especially it will have an advantage at any rate: either to learn about from an erroneous way, or to incite me to continue a wright one.

You will find a new term, in my Conspectus, viz. the "antherae sagittatae". I held this distinction necessary, because de Candolle states, the anthers in Euvernoniaceae to be "ecaudatae". But indeed they are truely caudate, the filaments being inserted on a higher region of the anthers, than at the base. The appendages however are of an other kind, than in the antherae caudatae of other Compositae. Therefore I call every anther sagittate, of which the appendages are the hollow, polliniferous lowest part of the anther itself, whilst the appendages of true caudate anthers are sterile, not hollow, and therefore not polliniferous.

I add the Botanical of April 15th, 1858, containing a small article about Elvira biflora, de Cand. and my Maria digyna.

I am, my dear friend, with the highest esteem and with the most sincere cordiality

Yours
very truly
Joachim Steetz Dr.

Hamburgh, Novemb 11th 1858.

My dear friend,

It is a very long time, since I had the pleasure of addressing a note to you, or getting a letter from you. — I hope you will have received my last letter of November 30th accompanied by a copy of Dr. Lee, Manns Compositae from Hongkong and the Botanical of November 1th, 1857, containing a small article about Dahlia Cincinnatis, de Cand. Presenting the packet, enveloped in a cross-band, at the post office there it was refused, because I had added on the outside of the copy to the address to you the only words: "with best regards from the author", and I was compelled to cut off the few words, when I would not miss the Steamer, which was on the point of going. Therefore I beg you will excuse the ill-treated copy.

Last summer I had the pleasure of receiving through Prof. Braun at Berlin two little but very interesting pamphlets from you, viz "Dr. Andersson's Synopsis of North-American willows", and your most important "Notes upon some Rubiaceae" viz your elucidation of Timonius, Bobea and some other Guettardeae, Coffeinum and the Vanguerieae Ixoreae, Morindeae, Psychotrieae and Coprosma, and I pay you my best thanks for them. I confess, that I am not well acquainted with the Order of Salicineae, and therefore Dr. Andersson's Synopsis will be a very welcome supply, when I shall examine the few species of willows, which I gotten from your country.

But your Notes on Tiaonius and Bobea, the by which you have settled the great confusion, hitherto existing amongst these genera, I have read immediately with highest satisfaction. Indeed, the whole of our systematical Botany needs more to clear up so many blunders, — slipped in by superficial and careless observations and unsufficient descriptions, by neglecting to examine allied groups, when novelties are to be published, merely by compiling and transcribing antiquated errors, without any examination, — than to make new species, which can be found, well ticketed in the herbariums, but not in the living nature. To clear up the value and limits of genera is the very question to be settled in our days. On this account I confess, that I am by no means satisfied with Miquel's Flora Indiae Batavae the whole Order of Compositae at least being a true compilation without any broad criticism, and most of the preten-ded novelties being now comprehended nor digested.

I promised in a former letter to send to you a Conspectus of the genera of the Tribe of Vernoniaceae which are treated in my manuscript of Dr. Peters Botany of Mozambique. My manuscript is now under the press, but not finished, because my leisure time, remaining from medical practice is but a very short one. Now you receive a brief copy of the conspectus of the genera, to which I have added the species, belonging to every one, so that you have the key, by which you can decide on them, because most of the species are known and enumerated in de Candolle's prodromus (under Vernonia, Decaneurum, etc.) except some news from Mozambique. When the whole manuscript, containing detailed characters of the genera and species will have left the press, I shall hasten, to send it to you, but I regret, that only the diagnoses and full descriptions are in Latin, all other remarks in German language, being commanded so for the whole work. — I must say, that the dereliction of Linnaeus golden rule, to treat every matter, belonging to Natural science in Latin language, being the Κατ' ἐξοχὴν language of learned men, is greatly hindering the advancement in science. It can be claimed from every learned man, to understand and to write well the Latin language; it is the claim for the Philologist to understand every living language, but not for the man of Natural science. That would be a question, quite impossible of course to fulfil. The custom to make use of the vernacular language in Natural History began in England, and now it is imitated by all peoples of our Continent, except the Russians, who in general are writing in Latin or French language. In this peculiar case I was engaged to follow the principles prescribed for the whole work.

The tribe of Vernoniaceae in de Candolle's prodromus is worked up in a less critical manner, than all others in the Order, because de Candolle at that time was not so acquainted with the particularities and properties of Compositae, than after having finished his most valuable and immense work. — Therefore I would not keep back this very small contribution, which is now more than a slight intention to begin a needful revision of this vast Order, to which your own numerous contributions in this way have incited me, to do a similar, and I am anxious to hear, what you think of my timid attempt. I beg you therefore, to say me your true opinion about the principles, on which I founded it. You have now, I should think, a good opportunity, to examine them, when, as I suppose, you will not have finished your manuscript of the Compositae of Capt. Wilkes expedition. No Botanist exist at this time who is so thoroughly acquainted with this vast Order than you, and your authority is notorious. Therefore a reprehension from you, will be for me of a greater value than a praise of any other, who has no idea of the difficulty to find the wright way in such a labyrinth of similar or analogous forms.

of that well known genus, and showing not any trace of a pappus, besides a little inconspicuous margin at the summit of the achoenium. Have you a specimen of your Tanacetum Chinense = Artemisia Chinensis Lin. fors.?

Many thanks for your fine specimen of Antennaria alpina, Gaertn. var. monocephala, from Behring's Straits. It may be the true Ant. monocephala, de Cand. because it has the acuminate inner scales of the involucre, as stated by de Candolle, particularly, if all your specimens are monocephalous. I have a single specimen of the same variety from Labrador also, but amongst others of the ordinary form, showing some aggregated sessile flower-heads. On the other hand all specimens (more than 20) of the Labrador plant, which I sent you under the name of A. monocephala, DC. were monocephalous, and therefore I hold it for de Candolle's true plant. I must confess, it has somewhat the habit of A. carpathica, Bluff et Fingerh., but it is a dwarfer plant, and the inner scales are more blunt, and not so acuminate as in the German specimens in my herbarium, and as it is stated to be in A. monocephala, DC. The comparison with original specimens in de Candolle's own herbarium only can remove every doubt, in this intricate object.

I am most anxious for your paper about Japan plants, because the Flora of that Island in every Order is so near allied to the North-American Flora, particulary of its east side, and indeed you are "the right man at the right place" of doing so, and more able as any one to work up a critical and useful parallel between both

Hamburgh, March 31th 1879.

My dear Professor Asa Gray,

Being upon the point of answering your kind letter, which crossed mine at sea, I was surprised and much delighted by receiving your second of february 16th. Nevertheless I was prevented to answer till now, being pressed by many business. To morrow our Steamer is going, and now indeed I cannot suffer any longer delay.

Truly both letters were most interesting for me in all points, and I became not a little proud, to learn, that my small paper on Hongkong Compositae has facilitated your work of Mr. Wright's Compositae of the same Island, and that in general your opinion of it, was so indulgent one. I pray you my best thanks for your valuable remarks on some plants contained in it. Attempting to reply upon them, I notice this:

The true Vern. Javanica, DC. is unknown to me. But I should think it different from Vern. coloraefolia, Beath., having the leaves entire, elliptic and acuminate, whilst in V. coloraefolia, at least in Dr. Leemann's and Mr. Fortune's specimens they are deeply toothed or sinuated or repand, very rarely entire, (sometimes the upper ones) and show of a shorter size and other form, viz. ovate or oblong and mostly blunt at the apex.

Aster striatus, Champ. I have not seen. But I have put aside for you a specimen of Aster Beathami, and A. baccharoides. Can you not return to me a specimen of Lagenophora Billardierii, quite unknown to me?

Amphiroapis leiocarpa, Beath., collected by Leemann has indeed the habit of Solidago Virgaurea so well, that I feel myself quite convinced, to incline in your

view, that it is no more than a variety of it with smooth achaenia. – But having not seen any specimens of the true genus Amphiraphis, I would not oppose Mr. Beathom, who had, as I suppose, Indian species of Amphiraphis at hand.

The species of Blumea troubled me likewise as yourself, because this difficult genus anxiously needs a revision by any one, who has the full stuff of specimens for comparison at hand. I suppose, that many of them are to be reduced. Some of those Blumeae, I had before me, viz. Blumea crinita, Arn. and Bl. Wightiana, DC. have been identified by my friend Dr. Leemann in Sir William Hookers herbarium, Bl. alata by me in my own herbarium; the others may be somewhat dubious, and recognized after the short diagnoses from de Candolle's prodr. and Bl. Javanica, Koll. after Zollinger's diagnosis in the Flora, l.c.

Verbesina prostrata, Hook. et Arn. I have not seen. I presume that it may be the same as my Wedelia calendulacea, Less., of which I have not seen Indian authentic specimens, the short diagnosis of both in de Cand. prodr. agreeing well with Less. mans plant. But it is a so good Wedelia, that I previous failed to inquire for it in the genus Verbesina. I have put aside a specimen for you.

Your character of Bidens Chinensis "acres of the achaenia erecta" is a very constant and characteristical one, and I confess gladly, to have quite overlooked it in my description.

My Gnaphalium purpureum, var. spathulatum, Lour. is surely quite different from Gnaph. Indicum, Linn., of which I have not seen specimens from Hongkong, but from India. I have still a specimen for you, and would be very glad, if you could procure me the form of the North American Gn. purpureum with loose floccose wool, being unknown to me.

The note to Gynura auriculata, DC. in my manuscript was more intelligible, than in the printed text. I had written: Hooker and Arnotts Gynura bulbosa and Gynura ovalis, DC. both not seen by de Cand. seem to be the same species, and not different from Gyn. auriculata, DC. These last words probably were held superfluous by my friend Dr. Leemann, who is an anxious adversary of every one prolixity; in this case however without any reason. So as it is printed, it expressed my view, that G. bulbosa and G. ovalis are identical, but not, that they seem also to belong to G. auriculata, DC., what I had in mind. The upper leaves in our specimens are indeed all auricled at their base. Original specimens from the Isle of Mauritius are also by me not seen, but de Candolle's short diagnosis in the Prodr. well agrees.

Of Artemisia lavandulaefolia, DC., (to which, as I presumed, may belong Dr. Leemann's two specimens from Shanghai, I enclose a little small fragment of my own single specimen for you. I should think it to be a true Artemisia, having the very habit

Now I come to your second letter. — I am much obliged to you for your brief opinion on my subject, which contains all, what is necessary, and I am much satisfied; for it you have expressed my own views. Not the Pollen alone can decide on genera! You will find, that even in a single case it was necessary to combine three species in one genus, of which one has the Pollen globose echinulate, two others polyëdrous echinulate, viz. in Guteabergia. Ltr. Sch. Pl. All other characters and the whole habit being quite equal in all three species, allowed not to make two genera of them. This was the only exception in the whole group of plants examined by me, embracing 16 genera and nearly 60 species, [illegible] of Vernoniaceae, which came under examination. In nature such exceptions from our principles in natural system occur often, and in numerous of our best Orders. — In all others of my new genera you will find a peculiar habit and many accessory essential characters, by which they are limited, so that I may hope, you would agree with them. I venture to state, the habit and the Pollen together were the Cynosure, (the leading star,) of forming them, and in no other case, than the single one of Guteabergia the two gave a diverging result. And indeed Vernonia and Decaneurum, as given in de Candolle prodr., are as unnatural genera, as are to be found in any other of the whole Prodromus.

No more than six sheets of my paper are printed now, and two others will finish the Vernoniaceae. I have asked our mutual friend Dr. Klotzsch

at Berlin, under whose redaction the whole work appears, for one copy of my treating of Compositae, to send it to you as far as it is ready, and then to send to you every sheet as soon as it appears. But I have no answer till now. Alas! no more then diagnoses and descriptions of new plants are in latin language, the motives, the explanations of my principles, and all other narrative matter are in German! — I may hope, you will find any one of your German acquaintance having the favour of interpreting it.

Dr. Ferd. Müller from Melbourne is a very active Botanist and will become surely for the next future a very able and learned one also. He has very sound views, and Australian Botany can expect many valuable contributions by him, a second Robert Brown perhaps, being still a very young man. ~~He has very sound views~~ I feel the greatest esteem for him and have also the pleasure of changing now and then letters with him. I can congratulate to you for his generous correspondence. —

Adieu my dear friend. I remain with greatest esteem and cordiality

Yours
Joachim Steetz

Genus: Pseudrosa, nov. gen. Involucrum calyculatum disco florum dimidio brevius. Involucri squamae obtusae. Stylus basi incrassatus basi in cavotum et apicem nectarii elevati soluti in tenuem ipsum recipiens. Achaenia obconica apice non attenuata. Pappus biserialis, exterior brevissimus. Facies Gymnocarpi tenuis generis prae se fert. Huc pertinent: Pseudrosa vulgaris. (Syn. Vernonia Pseudrosa) DC. prodr. 5. p. 32. N. 95. 2. V. volkameriaefolius. (Syn. Vern. volkameriaefolius, DC. prodr. 5. p. 32. N. 99.

Genus: Lychia, Ch. H. Schultz Bip. Involucrum calyculatum disco florum dimidio brevius. Stylus basi aequalis in nectarium parvum elevatum annuliforme immersus. Achaenia cylindracea apice parvulo perangustata. Pappus triserialis, exterior brevissimus. Huc pertinent: 1) L. vernonioides. Ch. H. Schultz Bip. in Plantae Schimperianae 2) L. globra. Sp. nova e Mozambique cum var. 2) canescentissima et var. 3) lana.

Subdivisio 4. Stearicideae, mihi.

Involucri squamis appendiculo foliaceo vel saltem scarioso terminatis. Laciniis corollae parte integra filiformi longissima multoties brevioribus. ~~Achaenia cylindracea basi attenuata to-costata.~~ Pappus brevissimus vel echinulato. Stylo basi tubero oblongo separabili immerso. Achaenia cylindracea basi attenuata 10-costata. Pappo triseriali paleaceo, exteriore brevissimo, interiore elongato inaequali cum setis paucis alternantibus.

Genus: Stearicida, Cassini. Involucrum late campanulatum disco florum paulo brevius. Involucri squamae appendiculo foliaceo scarioso tenuissimae basi in cupulam gamophyllam connatae. Pappus interior plerumque angustae paleaceus, interdum dilatatus et nervo medio obsoleto percurrens. Capitula ad cymam corymbosam depauperatam digesta, vel raro caulis monocephalus. Facies Decaneuri generis prae se fert.

Sectio 1: Species genuinae: Capitula semipollicaria, involucri triseriali squamae subaequilongae, exteriores appendiculo foliaceo viridi angustelineari elongato, interiores brevissimae auctae. (Stearicida, Cassini) Huc pertinet sola Stearicida Indica, Cass. (Syn. Vernonia callolepis, DC. Wild. DC. prs. 5. p. 61. N. 265.)

Sectio 2. Macrocephalae: Capitula 1–2-pollicaria, involucri 5–6 seriali squamae omnes, vel saltem interiores appendiculo foliaceo sagittato discolore lato oblongo auctae. (Stengelia, Ch. H. Schultz Bip.: Lech. L.)

Tribus Vernoniaceae, Lessing, de Cand.

Diagnosis tribus a cl. Candolleo in prodromo nunc capsa sub Vernoniaceae haec:

Calleae globosum echinulatum (vel spleridium granulatum vel breviter) echinulatum.

Subtribus 1. Vernonieae, de Cand:

Capitula discoidea homogama.

Divisio 1. Eurvernonieae, de Cand:

Antherae sagittatae. Calleae globosum echinulatum. Involucrum non compositum polyphyllum.

Subdivisio 1: Ethuliae, de Cand. l.c.

Pappo nullo aut simplici coroniformi.

Genus Ethulia, Cass: Achaenia obpyramidata 4–5 angula in involucro hemisphaerico subaequilongo aperta. Pappus coroniformis angustus. Discus epigynus magnus. Nectarium elevatum. — Huc pertinent: 1) E. conyzoides, Linn. 2) E. gracilis. Del. 3) E. angustifolia Roxb. cum var. β) falcifoliatoribus. hb. Drège N. 5065. 4) E. megacephala, Ch. H. Schultz Bip.

Subdivisio 2: Heterocomeae, de Cand.

Pappo uni- bi- aut pluriseriali, omnibus aut saltem interiore setiformi aut angustissime ligulato. Capitulis non fasciculatis.

Genus Vernonia Schreb. de Cand. (ex parte) Flores purpurei, rosei vel albi nunquam lutei; laciniae corollae parte integra subaequales. Stylus basi incrassatus. Pappus biserialis, serie interiore setosa elongata, exteriore paleaceo brevissimo. (Huc species plurimae in America tota boreali tam centrali tum australi indigenae.)

Genus Gymnocarpium, Cassini: Stylus basi aequalis vel attenuatus in nectarium valde elevatum cupuliforme immersus. Pappus uni vel biserialis setosus, raro exterior paleaceus. Flores Vernoniae.

§ 1. Species erectae. Cyma corymbiformis nuda. Involucri squamae obtusiusculae vel breviter mucronulatae.

2) Pappo uniseriali, ad setas paucissimis brevibus accessoriis. (Decaneuron Lagasca – non DC.) Huc pertinent: 1) G. cupulare, Cass. 2) G. quercifolium nov. sp. e Mozambique. 3) G. acuminatum (Vern. acuminata DC. prodr. 5. p. 32. N. 94. 4) G. Bollaeanum nov. sp. ex insul. Capitibus viridis. ~~5) G. fimbrilliferum, Cass.~~

β) pappo biseriali setoso, exter. brevissimo completo, interiore elongato.

5) G. fimbrilliferum, Cassini. (Syn: Decaneuron fimbrilliferum DC. Vernonia fimbrilliflora, Lessing. et de Cand. prodr. 5. p. 23. N. 49.)

§ 2. Species erectae vel scandentes. Inflorescentia paree foliata.

2) Scandent. Paniculo elongato laxo. Involucri squamae obtusiusculae vel breviter mucronulatae fuscae. Pappus uniseriali.

Huc pertinet: 6) G. obovatum, Gaudich. (Decaneurum obovatum, DC.)
7) ~~G. Abyssinicum, C.H. Schultz Bip.~~

β) arborescens. Paniculae pyramidatae supradecompositae compactissimae. Involucri squamae obtusiusculae vel breviter mucronulatae virescentes. Pappus uniserialis.

7) G. Abyssinicum, C.H. Schultz Bip. (Syn. Chelinsa Abyssinica, Schultz Bip.)
8) Erecta. Cyma corymbiformis. Involucri squamae obtusae. Pappus biserialis, serie interiore elongata setosa, exteriore brevissima paleacea.

8) G. cataractum. (Syn. Vernonia cataracta, DC. prodr. 5 p. 34 N. 100.

§ 3. Species scandentes. Rami floriferi axillares inparticulam foliorum plus minus ve aggregatum dispositi. Involucri squamae plus minusve acutae vel acuminatae, interiores valde elongatae sed discoflorum breviores, non tamen duplo breviores ac in caeteris speciebus.

9) G. vagans. (Syn. Vernon. vagans. DC. 5 p. 32. N. 96. 10) G. affinis. (exported) nov. sp. (Syn: Conyza vagans, Wall. cat. N. 3040. comp. 150.) (Prope Chitagonglacta, etiam G. vagante confusum.) 11) G. scandens. (Syn: Vernon. scandens DC. 5 p. 32. N. 97. 12) G. blandum. (Syn. Vern. blanda. DC. 5 p. 32. N. 98.)

Genus: Goagrothamnus. nov. gen. e Mozambique: Flores lutei! laciniae corollae parte integra duplo breviores. Styles basi in bulbum depressum disciformem desinentes; stigmatis rami papillis obtusissimis muniti. Folia triplinervia.
Species unica. G. divaricatus. nov. sp.

Divisio 2. Crystallopollineae, mihi.
Antherae sagittatae. Pollen polyëdrum granulatum vel breviter echinulatum. Involucrum non compressum polyphyllum.

Subdivisio 1. Gutenbergieae, mihi.
Pappo nullo.

Genus: Gutenbergia, C.H. Schultz Bip. Antherae niveae. Pollen breviter echinulatum. Achaenia ellipsoidea teretia nitida immarginata omnino calva in involucro multoties longiore abscondita. Discus epigynus angustus. Nectarium punctiforme. — Huc pertinet: 1) G. Ruppellii C.H. Schultz Bip. ~~4 G. Ethulia Ruppellii Richard.~~) 2) G. Perterii nov. sp. 3) G. longipes, nov. sp. (utraque e Mozambique.)
Obs. Gutenbergia Ruppellii, Sch. Bip. fructum polline globoso echinulato. Haec est unica anomala, quae mihi obvia fuit in tota Crystallopollinearum divisione.

Genus: Adenoon, Dalzell. Antherae nigricantes. Pollen minutissime granulatum. Achaenia cylindracea glandulis albis glabris undique sparsa. Species unica: Adenoon Indicum, Dalz. (Hookers Journ. of Bot. and Kew Gard. Misc. (1850.) T. 2 p. 344.

Subdivisio 2. Decaneureae, mihi.
Achaenia obconica 10-costata. Pappo setoso uni- vel biseriali setis omnibus in uniseriali, exterioribus in biseriali cito deciduis.

Genus: Decaneurum, DC. (ex parte vid. Leel. 1 in prodr.) Involucrum bracteis foliaceis cinctum discoflorum paullo brevius. Squamae involucri appendiculatae foliaceae terminatae. Pollen granulatum. Achaenia callo basilari destituta. Pappus uniserialis deciduus. Huc pertinent: 1) D. reticulatum, DC. 2) D. molle DC. 3) D. Courtallense (reject.) Dalz. Habitat haec mihi sunt nec ne vita.: D. frutescens. DC. D. Javanicum, Miquel Flora Ind. Batav. T. 2 p. 20. D. phylloleucum DC. pr. T. 7 p. 264.

Genus: Lyristemma, nov. gen. Involucrum bracteis foliaceis destitutum discoflorum dimidio brevius. Squamae involucri exappendiculatae. Pollen granulatum raristime breviter echinulatum. Achaenia callo basilari aucta. Pappus biserialis, exterior brevissimus deciduus, interior elongatus persistens. — Huc pertinent: 1) L. Deadrigulense. (Syn: Decan. Deadrigulense, DC.) 2) L. divergens (Syn: D. divergens, DC. prod. 5. p. 68 N. 12.) cum var. β) Nilgherrense. (Syn. Vernonia Nilgherrensis DC. pr. 5 p. 32. N. 93. 3) L. multiflorum. (Syn: Vernon. multiflora, Lour. DC. pr. 5. p. 31 N. 92.) cum var. β) Birmanorum. (Syn: Decan. divergens, DC. var. β) laxeolata DC. l.c.) 4) L. salignum. (Syn: Vernonia saligna et V. longicaulis DC. pr. 5. p. 33. N. 104 et 103. 5) L. pectiniforme (Syn: Vernon. pectiniformis et V. pectinata, DC. pr. 5 p. 31 N. 90 et 89.

Subdivisio 3. Xipholepideae, mihi.
Involucri squamis exappendiculatis. Pollen laevissime granulato fere laevi. Achaeniis teretibus 8-10-costatis. pappo 1-2-3 seriali setoso persistente.

Genus: Xipholepis. nov. gen. Involucrum cum volucratum discoflorum paullo brevius. Squamae involucri acuminatae subpungentes. Styles basi incrassatus. Achaenia obconica apice nonnihil attenuata. Pappus uniserialis. Huc pertinent: 1) X. Silhetensis. (Syn. Decan. Silhetense, DC. prodr. 2) X. aspera (Syn: Vern. aspera, Hamilt. DC. pr. 5. p. 31. N. 91. (non Lessing.) Vern. Roxburghii, Lessing. Linnaea. Tom. 6 p. (1831.) p. 674 N. 130.

Huc pertinet: 1) A. mossambicensis, nov. sp. 2) A. Richardi. (Syn. Vernonia macrocephala, A. Rich. in Tentam. flor. Abyss. T. 1. p. 373. tab. 57. (non Less.)) 3) A. Adoënsis. (Syn. Steagelia Adoënsis Schultz Bip. in Schimper pl. Abyss. 4) A. Kotschyana. (Syn. Steagelia Kotschyana, Schultz Bip. Hochst. mss. in Kotschy pl. tubic. Vernonia Kotschyana Schultz Bip.) 5) A. Buchingeri. (Syn. Vernonia (Steagelia) Buchingeri, Sch. Bip. herb. (zeae diagnosi.) Schimp. Abyss. Sect 5 N. 386 distrib. Buchinger.

Genus: Achyrocoma, Cassini.: Involucrum campanulatum disco florum paullo brevius. Involucri squamae appendiculo parva scariosa colorata terminatae basi liberae. Pappi interioris paleae dilatatae manifeste uninerviae. Ad flores centrales ~~totus~~ modus ignotus. (Genus minima in America boreali, sed in Senegalia et tota verosimiliter Africa occidentali indigena. Achyrocoma! tomentosa Cassini. (Syn. Vernonia nana, Schultz Bip. in herbario zeae diagnosi. Vidi capitula pauca bene conservata, sed desunt folia et caulis.)

Genus: Steagelia, Cass. Schultz Bip. (exporte zeil. Sect. 2.) Involucrum turbinatum disco florum dimidio brevius. Involucri squamae appendiculo foliaceo terminatae basi liberae. Pappus interior aequissime ~~dilatatus~~ paleaceus. Capitula in corymbum corymbiformem confertum disposita. Huc pertinent: 1) Steag. controversa, Sch. Bip. (Syn. Vernonia Abyssinica Sch. Bip. in Schimper pl. Abyss. 2) St. Schimperi. (Syn. Vernonia Schimperi DC. pr. T. 7 p. 264 et 266. Eupatorium? in schedula plantt. Schimp. in Arabia felici lecta N. 868.)

Subdivisio 5: Eucrystallopallineae, mihi Polline minutissime granulato fere laevi. Pappo biseriali, exteriore late paleaceo brevissimo, interiore setoso elongato.

Genus Crystallopollen, nov. gen. Involucrum regulariter imbricatum. Flores purpurei vel rosei. Laciniae corollae parte inferiore duplo breviores. Thylus basi incrassatus. Achaenia 4–5-gona, angulis costata. Facies Vernoniae generis proae se fert.

Huc pertinent: Cr. aequifolium, et Cr. latifolium, ambo nova spp. e Mozambique.

Genus: Amborella, Steetz nov gen: Stylus basi bulbo depresso dissiformi auctus. Achaenia cylindracea, 10-costata. Reliquae Crystallopollinis. Sola species mihi nota: Amborella Hochstetteri, (Syn: Veronia Hochstetteri C. H. Schultz Bip. in Lehmpl. Pl. Abyss. Lech. 1 N. 255.)

flowering with its solitary, axillar red flowers and yielding after ripe legumes. (Probably the first bud of C. purpureus, Lesp. germinated in a much earlier time, but was overlooked by the Botanist of the C. Adamii, Hort. in whose summit it arised.) Some years after the whole tree perished in a hard winter, or perhaps by another physiological cause. — I never will forget the high impression of this singular fact, by which it proved the endeavour and faculty of nature to restore at last its own laws, disturbed by human hands during an interval of more than 20 years, by the vitality and perseverance inherent to few grains of pollen unnaturally combined with the ovules of a different species, and so to perform the full regeneration of both species on the same stem of its Bastard. Every time, when I visited this interesting tree, I was struck with admiration. The same facts happens often by cultivators, who produce fruit-trees. Seedlings of good sorts of fruit not seldom are running back into its wild species, and elected sorts are better preserved by cuttings and grafts, than by seeds. On the other hand many of them, for example delicious kinds of apples and pears, cultivated in our country everywhere, 40–50 years ago, when I was a child, disappeared totally since a long time, whilst wild apple-bear- or plum-trees occur unchanged every where, particularly in seed plots.

Quite the same fact occurs under cultivation of domestical animals, which abounds, it is true, in significant races. Every breeder knows, that it is very difficult, to preserve good races, and that domestical selection needs the greatest accuracy. Nevertheless many distinct races of dogs and other cattle for breed evanished under our own eyes. Disappearing of good species of animals of the last period in its wild state is not proved till now, if besides the Dodo, the known but very dim fact. Myself I have but few a little experience in this regard. But Andreas Wagner published just now a treatise: "On species, with regard of Natterers's, Darwin's, Isid. Geoffroy's and Agassiz's views", in the: "Sitzungsberichte der Königlich Bayerschen Academie der Wissenschaften zu München, 1861. Tom. I. Heft 3. p. 308." — There he reviews the cases of fertile Bastards, enumerated by Isid. Geoffroy, and comes to the result, that most of them, besides two or three are very doubtful. He affirms, that every Bastard by selection of his own is sterile, but often fertile, when brought together with one of the parent-species. Therefore he maintains, that the fertility of Bastards produced from hare and rabbit, raveled

Hamburgh, October 19, 1861.

My dear friend,

At length comes your slow Hamburgh correspondent to you, asking your pardon for so long a silence. Indeed it seems to be very ungrateful, that I received so many interesting papers from your hand, without answering them. But, believe me, my dear friend, I was oppressed by with many various business, so that my whole correspondence was interrupted, and that I felt it necessary to escape in the country air and in a "Dolce far niente" for a few weeks, to restore my health being in the point of becoming infirm. There, "procul negotiis", my hypochondrious disposition left me quickly. Only then I found the leisure of reading Darwin's celebrated Treatise (2 edit. translated by Bronn) and of becoming familiar with it. Only then I had the high pleasure of studying your "Free examination of it". I say "studying", for I am not so familiar with your language, as to be able to read it without the help of a dictionary, particularly the first pages, which however were pleasing enough to me, when I understood them fully in a second reading.

I found, what I expected, that you are by no means the real defender of Darwin's theory, whenever it may seem so. You had only in mind, to take in protection a theory, explained in a scientifical manner, and written in a decent and reputable style, against the calumniation and detraction of some hyperorthodox reviewers of North-America and against the exuberance of the materialistic and atheistic school of our continent. That you have done perfectly in a most intelligent and indulgent manner. And because you held it worth the while, to defend Darwin's theory in that way, I conclude further, that Hyperorthodoxy is widely spread over your country, and that your people is more dependent and subjected to be bigotted Clerus than in Germany. Indeed in our country Darwin's theory has not to fear insults from that side, but, if I am not mistaken, it will be more disproved much more than approved by our naturalists, particularly by Zoologists, in a scientifical manner. I will not say, that Germany abounds with Atheists or even blasphemers, in contrary, we have also many orthodox theologists, but their influence on the people is but very little, and they risk not to manifest their own beliefs and dogma's in a zealotic manner. On the other hand I cannot but regret, when I must confess, that a great

of our unlearned people is indifferent, whilst the learned one proves to theism or atheism. I could conceive, that a Zealous naturalist and a learned physician want to be an orthodox believer, but I am unable to conceive, that they could be indifferent and wanting of a deep religious mind, in the best sense of the word, founded on the very adoration and veneration of all wonders around us, which we see every day, but which we cannot unriddle at time; and founded on the consciousness, that all our human work is mostly imperfect in comparation of divine order and creation. Such a vivid veneration is not supported in us by believing, but by clear seeing. Therefore in all things, concerning natural, i.e. divine institutions I am Optimist in the strongest sense, in spite I will never be able to comprehend some of them, for example: the permanent war in the whole organic world. I see it; I could wish, having it avoided in the design of creation; it could not; and knowing nothing about cause and design, I feel myself incompetent, to criticize such general institutions. In brief: I cannot believe, what I don't see, or what not is probable to my mind; but, because I see many wonders really passing, without having the faculty of unriddling them, it would be illogical to deny them in both cases. For instance: I see not the the trinity of God, nor I perceive his personality, thought with human affections, and some other dogmas of christian religion; I cannot believe them; they let me indifferent, because I feel incompetent myself to deny them. On the other hand: I see the fact of strong connexion between spirit and body, or, what we call the nature of vital power, but I see not how they are connected; to deny it therefore, would be ridiculous, because it treats not believing but seeing. I love Christianism, in regard of its moral, being the sincerest noblest and most natural one I know, I venerate therefore its high, clean and faultless founder, not as God himself, but as man born with human affections, and become by his own faculty, energy, abstinence and perseverance to a spiritual perfection never attained by any other. That I can comprehend, not his divine transfiguration.

These and similar considerations lead me indirectly towards the subject of Darwin's theory in his treatise. Whilst their question is quite a matter of natural science, Darwin's attempt to decide it surpasses beyond its limits and even those of the historical one. So far as I know I think, ours science, because it is founded upon facts, perceivable by our own senses, and because it endeavours their real knowing and not believing, it is restricted into very close limits; there are the exact observation and cautious experiment in a most elastic sense. All inferences founded without these or beyond, belong to philosophical science and are supernatural hypotheses, not proved by facts. In this case I regard Darwin's answer, because his ultimate suggestions, (the fundament of his theory) extend far beyond historical time; they lie beyond the horizon of our mind, and are inaccessible for every observation or experiment. Moreover, Darwin's theory, giving the perspective that creation was effected only by one primordial species of being, and continued by diversification and adaptation till our time; — may be conceded, nevertheless the first act of creation of this one first organic being is by no means cleared up. Therefore I regard Darwin's answer as a vain and vague proof and of little advantage for science. The question: either primordial or derivative creation of organic beings will perhaps never (as you well remark,) be absolved, and its answer, in time at least, is a premature abortion, whilst so many important questions in Physiology, which are to be explained by observation or experiment lie before us unabsolved. No wonder, that Darwin everywhere met with unsurmountable difficulties, by which the chain of his hypotheses is interrupted.

Moreover the whole exploration of his theory wants of every strictness and accuracy, which is to be claimed so cautiously in natural science. Many facts are recorded on the only authority of popular belief; others of course on authority of learned men, but without any critic, often they are the results of an occasional discourse. And such superficial and by no means undoubted and proved facts Darwin claims to be held true by men of science! — On the other hand Darwin passes in silence over many facts well proved by experiment, which mostly invalidate his theory. He witches anxiously the well known and often observed fact of the energical endeavour of nature to return in its primitive species by hybrid plants. You know the cases of Cytisus Adamii, Hort. (hybrid of C. Laburnum L. and C. purpureus, Lesp.), thrice observed only in Germany, of which I observed the most interesting one in our botanical garden many years ago. There vigoured C. Adamii, Hort. (a tree 12–14 feet in height) long time, flourishing every year, but never yielding any legumen. Once appeared in one racemes amongst flesh-coloured flowers a single yellow one like those of C. Laburnum, L. and yielded one legumen with ripe seeds. In the nexteat year more yellow flowers in more racemes were observed, till in the next following 2 or 3 years the number of yellow flowers by degrees increased (under the same condition of yielding perfect legumes) and at last entirely the flesh-coloured flowers of the hybrid. At the same time, in the summit of the tree a little bush arised, but was not attended to, till in two following years it increased and was recognized as the true C. purpureus, Lesp.

I cannot finish my long letter, (for which I sincerely beg your pardon) without expressing you my high regret about the dreadful war, by which your formerly so happy country now is disturbed. With the greatest sympathy for your Northern States, in which science and arts in our century raised up so widely and quickly to such a height. I look avidiously for daily news from the trans atlantic mails. May be removed soon the cruel war between brethren and may peace soon return in your so hardly vexed country!

And now, my dear friend, fare well, and let hear from you something, which can quiet my sorrows about your well-being. Besides I long for some lines from your hand, which I missed since more than a year by my own fault, and do not render like for like.

Believe me, my dear friend

every yours sincerely and cordially

Joachim Steetz

by Roux at Angoulême, may not be doubtless, because it is not clearly proved; and he supposes, that the propagation, of which is said the most fertile one, viz $\frac{1}{4}$ rabbit and $\frac{3}{4}$ hare cannot be the propagation of pure Bastards in a direct line.

But I tell you things, which probably are better known to you than to me, and it will be time to finish, when I shall not abuse your patience; moreover as the intricate matter in regard can by no means be absolved in a brief letter. Yet allow me, to add my own objections in some principal views against the probability of Darwin's theory, as far as he claims it not only adapted for derivation and diversification within the period of which we know ~~only~~ nothing but the fossil vestiges of a perished nature, (what I let undecided) but also: for containing it also in the historical time till our now living nature, under our own eyes. The arguments which make improbable to me the extention of Darwin's theory till our present time, might be these:

1) Where ever we see species, inclining to variation, this is always the exception not the rule, because in wild state the primitive form of species prevails.

2) When a species is variable, we observe the variation occurring in various direction and different degree often together in many forms, so that it is more difficult to define varieties in strict diagnoses or limits, than species. Therefore they may not be beginning new species.

3) When a variety depends from a peculiar kind of soil, for example from lime, salt, granit, dolomit etc. such variety is more constant and significant than another become verified by a more accidental cause. — When brought it in a different soil, we observe it either perishing, or going back to the primitive form. In the first case it may rank with a good species, in the second as a mere variety, but never it will be a beginning new species, in the sense of Darwin's natural selection.

4) In ~~wild~~ nature we see many fold struggle for existence and the general war ~~is~~ between organic beings is so extended and perpetuated, that it seems to rank with a natural law, but never in the sense of Darwin's theory for adaptation, because in spite of it organic nature (remains) in the same mutual proportion, at least during the last century. It is never observed, that in this space of time one species was oppressed or extinguished by another. When however plants or animals evanished in one or other locality, it may have been the consequence of human cultivation or annihilation. Entire annihilation of a good species is, as far as I know, never observed.

5) Hybridization in wild nature is a very seldom fact. Whereever we met with it, the Bastard always remains in minority, whilst his both parents abound around it. According to Darwin's theory a Bastard once arisen should be thought to be persistent, when not prevailing. But in many cases we see it quickly disappear in wild nature. I may add, that in the last time the supposition of spontaneous hybridization will probably be exaggerated by some systematists.

6) When Darwin requires for diversification of species by natural selection an indefinite interval of time, expecting favourable circumstances for it, this claim appears in a very strong contrast with the experience, that diversication by human aid, or by domestical selection is produced in the greatest multiplicity in very short a time. But when diversification by adaptation should be design by creation, it might be not reasonable to assume, that it should be committed to accidental causes in long intervals, whilst it is applicable by human aid at every time. This leads

7) to the often uttered objection against Darwin, that Bastards not are fertile with its own in infinite, which however, so far as I know, never is disproved by exact experiments, whereever I know, that by many authorities, viz by Isid. Geoffroy, Morton, Agassiz and others this objection strongly is opposed to.

Darwin's theory therefore seems to me, (as also to you) being supported in general on the imperfect state of our science, brought for and continued by the breach among naturalists about the circumference of our Classes, Orders, Genera, Species, Varieties etc.; by the faults within our natural system, which is for (real) off, being a natural one; by the want of uniformity in observing, examining and illustrating organic beings; by the every year increasing of the extent of scientifical matter and by the incapability to subdue it at once; etc. Darwin's theory in deed will remain as long undecided, as long naturalists endeavour remains unable to reform such failures; and the next task for naturalists will be to fix more precise limits for species, varieties, etc by more exact observations and long continued cautious exerted experiments. Then only it may become possible to bring to end the very important question, if Bastards conjugated with its own (are) ~~is~~ fertile in infinite or not, and what when the last should be proved as true, to exhibit this fact as the best, because constant criterium for the limits of species. With the decision of this question Darwin's theory will fall or stand, but surely not in so short a time; as Partheanogenesis in plants and Faure's theory of transmutation of Aegilops in wheat are known away. For ~~it~~ ~~cause~~ the present naturalists will do good, to sheath the sword till that, according to my opinion, still very remote time.

My elaboration of Prof. Peters Compositae from Mozambique is not yet absolved by me. The twenty first sheet just now is gone under the press. Certainly you will wonder for such a slow progress, and for the unproportionated increase of the whole. The first you may excuse by my very scattered time, the second by the circumstance, that I have taken in consideration many adjacent genera, and closely allied species, so that you will find some small monographs of any intricate and confuse little genera in the notes. Therefore I was stopped often by the examination of ten or more species, when I had in point to define one.

UNITED STATES NATIONAL MUSEUM

UNDER DIRECTION OF

Post Address: THE SMITHSONIAN INSTITUTION

WASHINGTON D.C. May 19th 1884.

Prof. Dr. Asa Gray.

Dear Sir: -

Having just received your manuscript notes on 'Plants collected on the Commander Islands' I take the first opportunity of thanking you for the labor you have bestowed upon it. It was very gratifying to me to learn that the collection was of some interest, so I devoted much time to it. The climate was very damp, however, making a proper preservation of the specimens next to impossible, hence the lack of several species which had to be thrown away.

Examining the herbarium sent to the Agricultural Department I find that the type specimen of Cassiope oxycoccoides is in your possession.

I would be very obliged if you would consent to sending it on for my inspection. By seeing it I may possibly be able to tell the exact spot where the specimen was taken. I left paper on the islands, and have the hope that the wife of the governor, an accomplished lady, is making additional collections for me. I might, therefore, be able to procure more specimens, if I can direct her attention to the place where it grows. It is my intention to write as soon as possible, sending at the same time a sketch of the new species, and also of Bryanthus Gmelini – the very locality of which I can easily describe to her. I therefore think it important that I be allowed to inspect the specimen, which then will be duly returned to you. By the way, is it described in detail anywhere? If not, might it not as well be described in connection with my report?

I propose to append a catalogue of the plants, indicating the localities, dates, altitudes, flowering time, etc, as far as my notes go. I suppose they may have some interest to future collectors.

Is it too much to ask you to give me for an introduction as brief account of the general aspect of the flora, as shown by my collection, especially its relation to the flora of Kamtschatka on the one hand and the American Aleutians on the other? I will add that Angelica Archangelica, Cochlearia fenestrata, Empetrum nigrum (of course) and Elymus mollis are very common, and that I found Rosa Kamtschatica, an Urtica and an Aconitum, all of which were collected but got spoiled by the dampness.

I am, Yours, very truly Leonhard Stejneger

Smithsonian Institution
Washington D.C. Oct. 31. '84.

Dear Prof. Gray!

It has been quite impossible for me to reply to your kind letter and return the type specimen of Cassiope stycocoides. This I will manage to do today, sending you at the same time the notes I possess regarding the plants I collected. I should be very much obliged, if you would favor me with your opinion, whether these notes are worth publication or not, and any suggestion in regard to them would be very welcome.

As so long time has elapsed I take the liberty to quote the following from your letter: "I will supply a character, and some notes of the Cassiope.

When you come to want them send me back my notes and I will write something about the varities to add to the interest of your report.

Accordingly I also send your manuscript, very gratfal for your kindness.

No collection have yet been received from Bering Island this year, but I hope, that they are on the way. Otherwise we must have patience until next year.

Very truly Yours

Leonhard Stejneger.

24 Bloomsbury Street
London

March. 20th ~~Feb 20~~ 1853

Dear Sir

I ought long ago to have acknowledged your Letter (not dated) but I was expecting that I should have found buyers for another Set or two of the Plants I have but have failed up to the present time I have only 500 fine, the great objection appears to be the smallness of the Sets, I took 4 of them to Paris but there found they were all supplied. — I enclose you account for them Sold, you will see a balance due to you of £8.15.—

I have lately received from Swan River a small collection of dried Plants about 70 or 75 Species which I have named & am about putting them into Sets @ 5/- pr Set, could a Set suit you or any of your friends in America, there are also some Seed Vessels of Banksias &c. &c. that

enclosed which are very beautiful, enclosed is a list of them price £1.1 the lot, they are very suitable for Botanical Museums &c. they are neatly in different stages & as you will see I give several specimens.

Trusting you will excuse the long delay in sending off this letter

I remain
Yours very faithfully
Samuel Stevens

Dr. A. Gray

Dr. A. Gray in acct with S. Stevens

1852 Dec 14	To Commission & bill of Dried Plants & Seeds Rec'd £10.12 Balance	2 4 —		1857 Jany 9	By Sale proceeds of Plants, Seeds &c Rec'd	3 15 —
		8 15 —		Dec. 27 1852	By do of bill of Exchange Rec'd & Sent Physician Hamburgh	1 10 —
					By do do do do	3 — —
					By do do do Sent Physician	1 6 —
					By do do do do	1 8 —
		£ 10 19 —				£ 10 19 —
					By Balance	8 15 —

D.r A. Gray

Office of

U. S. Geological and Geographical
Survey of the Territories,

Stevenson

Jany 4th, 1876.

My Kind Professor Gray.

The little package of plants came safely to hand, and I am unable to express to you how highly Mrs Stevenson and I appreciate them. We prize them particularly because they were collected by ~~the hands of~~ Dr Hooker and yourself, and I think from the exquisite manner in which they are arranged the kind hand of Mrs Gray took part. I am very grateful for your kind remembrance, and shall always hope to have an opportunity to serve you in some way again in the future.

With the best wishes from Mrs Stevenson & myself to both yourself & Mrs Gray.

I am Dear Sir Yours truly
James Stevenson

Friday
181 Sloane St

Dear Mr Gray

I am so very sorry to have missed you —

I want you dear & particularly would like you to think it over while to come to an afternoon tea of my daughter's & I are giving together on Wednesday next from 5 to 6 o'clock at her house 44 Hans Place (close by)

because it is Easter
Mon my Dear
it comes to me as true pleasure
to see you, & you would meet
from [illegible] Dear Cousin Josie
Love from Aunt Hattie

Harriet Stewart

Tuesday night
101 Sloane St

Dear Dr. Gray — I have yr
card — I do beg & entreat — &
if need be insist on your all
coming to Luncheon at 2 o'clock
on Thursday —
& pray do as I wish
Ever yours
H Stewart

Tram	0,9
Elk Burb.	1,12,0
Watch	1.
Lamp —	1
Drinks —	.3
Lunch	1,6
Cabs —	1,6
Tram to Kew	2,0

Peoria Ills
March 29th 1869

Prof. Asa Gray
Sir

Thinking it may be of some interest to you I herewith send you a specimen of a variety of Stylosanthes elatior which I collected in northern Miss. in 1863 & which I have not been able to find an accurate description. In Chapmans work it is not noticed at all.
It may be var. hispidissima of Michx. but if it is it is not well named. I have not seen his description of it. It is noticed in Gray & Darrys Flora of North America but not. This variety is very well marked. I found it only in Tishomingo Co Miss. in the vicinity of Iuka twenty two miles east of Corinth. The country there is rather barren and hilly covered mainly with the Pinus mitis I presume it grows in many other parts of the South. Dr Brendel of this place says he found it in Southern Ills
I observed the Stylosanthes elatior very closely as it ordinarily grows in northern Miss Tenn. Ala. Ga. and South Carolina. It is mostly

erect, often ascending, sometimes decumbent, rarely diffuse ~~while the~~ It is not ordinarily tufted, while the variety is often tufted, often diffuse, usually decumbent or ascending, never upright, whole plant a full half smaller, flowering later, I should think full two weeks.

I had intended to send this with quite a number of species I collected in the South ~~to~~ that are not in Dr Chapmans Flora of the Southern States, to him, together with some note I made on the flora of the southern states, but I can get no communication from him.

Respectfully

J D Stewart M D

Garden and Forest.

Tribune Building, New York.

Dec. 2d, 1890.

Dear Mr. Watson:-

I have a communication from Mr. Munson which gives the result of some correspondence with French botanists concerning Vitis rubra or V. palmata. Knowing so little of the matter I have not even read it carefully, but believe that he does not consider this a question of priority of name but of distinction between specimens, or something of that sort. I wish you would tell me whether he makes a case against the name V. palmata - that is, whether we did right in changing the name of the vine which he called V. rubra to V. palamta. If you will please return the enclosed to me with your opinion I shall be greatly obliged,

Yours very truly,

W. A. Stiles

Mr. Sereno Watson,

Cambridge, Mass.

C. S. Howe

Oxford Dec. 14 1865

Dr Dr.

Yours of the 6th duly rcd.
I thought you would like Mohl—
we must not not let Schleiden have
the honor of the prior discovery of
the theory, if it belongs to you. —
But I must take the liberty to put
in a caveat against yr. calling
these movements 'self-caused'.
Pray how does it appear that the
light is the condition & not the
cause of the movements? All
that appears from Mohl's observations,
as far as I see, is that when the
light strikes the leaf, then the
change takes place. To be sure

I dont think Mr. Mill has succeeded
in identifying it with a condition.
Confuse matters as we may, a cause
remains something very different,
toto coelo different from an invariable
antecedent, just as much as it differs
from an inseparable concomitant,
and that's the reason why people now
adays find it just as impossible as
Hume did to lay their finger upon
a cause in the external world,
though as soon as any one understands
the difference between laying his
finger on a thing, and having it
laid on a thing he will see that
what 'cause' means.

Will his Honor allow the plaintiff
to enter up judgment in his favor, against
the defendant — costs to abide the event?

Very truly Your pupil

Chas. S. Francis & Co.

it doesn't result from this that they are not self-caused, but then neither does it follow that they are; and the onus probandi fairly lies with you to show that it is merely cum hoc, & not propter hoc. And it will hardly help you to insist that the cells depend under other influences, for these would only be so many more conditions for you to show to be conditions & not causes. In fact how can you distinguish between these conditions, these sine quibus non, these indispensable requisites, and verae causae?

And besides there is "another alternative": may not these movements be each the several result of the joint action of the light & of the cellular power? Isn't it a dilemma with three horns?

We may not be able to explain what a cause is, - in fact I don't think any explanation can be given, for the excellent reason that by ultra terms of the proposition it can't be made any plainer; it's an ultima Thule; we can't get any farther, any more than we can tell Mr. Sophocles where his arm would be if he stretched it out when he stands on the confines of space* — still we can say & show what it is not. And

* When he does stand there I shall be able to answer his question!

Stone

(Βοσσφόρος) Ox-ford Feb. 24. 57

D. Dr.

I send a very meagre attempt at an outline of Jessen's paper you sent me. It consists of a comparison of a great many facts, & so cant be abstracted with any great justice to cl. auctori. From the fact that he rec'd. the prize some people might infer that the Essay must be a valuable one: I think, however, that this is a non-sequitur, unless it is first shown that the Academy ~~only~~ gives prizes to valuable essays only; or that the competitors wrote valuable essays. Nest-ce pas?

If you want a fuller outline, of course I am at yr. service, if you dont want it very soon.

Maple sugar time is not far distant, judging from the mud — shall you want any, & how much?

Nothing new in anything but law. — Cant I get a copy of Agassiz's Introduction? or must one "go the whole turtle"?

Please remember me to Mr. Hochstein — has he got over that pun of yours yet? That "rébus" as he called it.

I turn over the leaf to send Mrs. Gray my best regards — & my best to yourself, too; — or rather, as that seems to be a contradiction, perhaps leave out the superlatives, and be simply very positively

Truly your pupil

Asa.

Prize Essay.

Question I "Is the duration of life of the veg. ind. in the broadest sense (i.e. in Gallesio's) indeterminate; or is it limited & subordinate to the duration of the species?" [It is limited; & probably subordinate]

Qu. II "Is the duration of the life of Vegetables developed from seed & increased by non-sexual reproduction (by the formation of shoots or offsets of what kind soever) indeterminate, ceasing ~~able~~ accidentally or from unfavorable external circumstances merely, before the cessation of the species — or is it limited within determinate bounds?" (a question of kind not inds. — yes: limited).

A. Theoretically considered.

65. As to Qu I.

What is meant by "duration of life"

That veh. is terminated by the end of life

i.e. by death.

Life is the power of independent self-nourishment of growth & of death. (Aristotle) A physiological definition.

66 Anatomically: living beings are those employing organs.

Organs are cells, & their composites.

Living them means: formed of cells and transforming itself by an inherent power

67 Life is self dependent i.e. independent

Its activity, to attain any definite end, must be governed by some definite idea.

68 Death — the end of life — occurs

1, When this "idea" is fully realized

2. When nutrition (& growth) cease.

3 When the connexion of part is destroyed.

In the 1st case death will be general & will affect the whole organism; then nutrition & growth

will cease, & at last decomposition will
ensue.
But here death - like development - will be
gradual. The plant dies because it has
grown too old to live any longer.
In cases 2 & 3 death is the result of single
'destruction' of the several parts or their
powers.
Here a partial injurious disturbance
spreads until it becomes at last total.
59 Cases 2 & 3, or admitted by all may occur from
external causes.
Case 1 - death from old age - the Author
admits; those who deny it hold their
doctrine in connexion with another
of veg. viability. But this view is excluded
by the definition in qu. I.
Besides:
Sexual reproduction
Here a new independent being is
produced, - by the united action of two
organisms, - developed partly in the interior of

one of these, – separate from it by its own growth, – & then becomes independent.

Non sexual reproduction.

Here parts of a living organism produced as just stated, – by their own development, – are enabled to live independently, – after having been connected with the parts out of wh. they immediately spring, – during their first formation only.

There are resemblances, – in their production.

As to their products they differ

If non-sexual repr. = sexual the first must produce a new plant different from its mother.

But it is not =; for

non sex. repr. is our only means for keeping a kind from becoming dissimilar.

Hence non sex. repr. is only an iteration of the old, not a production of a new being.

Now no created being can have an infinite duration [Sed quere de hoc: it can have no infinite duration a parte ante; but why not a parte post?]

Besides this is contrary to observation.
Hence two individuals have a limited duration
(for non sex. repr. begets no individs. (supra).

As to relation of ind. to Specific life
Species maybe known by their hybrids not
being productive.
The Qu I defines individuality.

. As species of the Past have died out so those of
the Present will; but the Author cant wait
till they do, so he leaves this question unsettled
& devotes himself to the ind.'s duration of life
He has shown that theoretically this duration
has an end, but observation must show when
this end is.

B. Practically.

Answers Qu II.
1) Is the duration of life of Veg. produced from
seeds unlimited or limited (the latter)
2) Is the duration of life of Veg. increased by
non sex. repr. unlimited or limited (d)
3) Is the limit, if it exists, determinate for each
Kind?

C. 1. On the duration of the life of Veg. produced
from seeds.
C. 2. On diseases & old-age-weaknesses of Species
a) deciduous b) non deciduous
c) the nature & extension of diseases
(herein 1) gen. phen. of disease
*2) gen. view of cases of disease.
C. 3. Individual duration of life of single
Species. (potatoes 50-60 years &c).
Addenda.

*Species of plants are struck by disease or
sudden death; the ground thereof being age.
ex. Calceolariae; Tropaeolum speciosum (Poppelsd.
all fruit-trees; potatoe. &c
Another with the greatest care die out, although
their equal age cannot be shown.
Hyacinths, Pelargoniae, Chesnut &c
further- die of very similar diseases merely
from inadequate nourishment

as melons, Aegopodium Podagraria (L)
Another series; here it is doubtful whether inter-
nal or external causes produce their dying out
although in many of the cases adduced the
external cause is difficult to determine or
not at all. Hops, Hedychium flavescens
Prunus Laurocerasus.

WORCESTER NATURAL HISTORY SOCIETY,

11 Foster Street,

Worcester, Mass., Dec 6th 1880

Mr Benj L Robinson -

Dear Sir:-

I received your postal of Nov 28 giving the names of the plants for which I am ~~greatly~~ obliged -

Can you tell me where the discription of Sclerum atropurpureum can be found.?. The Moss which you thought was Fontinalis Novae Angliae cannot be correct -

I should have informed you that the leaves are costate - I wish however you would compare it with Dichelyma P 272 but more especially with D. Swartzii P 275. Lr. Manual & also with Hypnum fluitans P 383 and varieties

Hoping this will not tribulate you to much - Respectfully Yours

G. E. Stone

Sol. atropurpureum
DC. Prod. XIII, i, 242.
Fl. yellow.

Store.

BUSSEY INSTITUTION
(OF HARVARD UNIVERSITY)

Jamaica Plain, Mass., 25 June 1875

My dear Sir

We are led to believe that some confusion has arisen among botanists from their ignorance of this fact that the awns of H. lanatus curl over on drying.

I make bold even to ask, though with many doubts and solicitations - whether in the light of your fact the Holcus mollis of the Europeans can stand as a true species? [It is said to affect sandy places].

I am

very truly

J. H. Steere

Dr. Asa Gray
Cambridge

Our here on the frontier we have to turn our hands to all sorts of work, - from carpentering and plumbing down. In this sense I have had the impertinence to make a botanical observation! - aided & stimulated say by our

"freshman class of one" — an acute connecticut lad named Ford.

We find namely that the arms of Helens lanatus which you (in Botany of Northern U.S. p. 642) describe and figure (Tab. XII.) as "bent" and "curved" is really straight in fresh specimens and that it curls over when dried rapidly. Indeed an putting separate plucked-off flowers in the sun we find that the arms curl over before our eyes and assume the position which your figure represents. In some cases we have seen the arms curl outward and at others inward.

From the figures in the books relating to grasses which we have here,

Monroe, Wis., Mar. 11, 1891.

Dear Sir: -

I am a young fellow who has been working at botany for several years. I cannot find descriptions of two plants which have been found in Wisconsin, in Gray's Manual. The plants are Mitella caulescens, Nutt. (found in Manitowoc county by T. A. Bruhin) and Verbena xutha, Lehm. (found in Grant county by T. A. Bruhin). Please send me descriptions. Dicentra eximia, D.C. (found in Grant county by T. A. Bruhin) is not credited for Wis. in the latest revision of the Manual. I enclose stamp for reply.

Yours.

Steve Stuntz

Monroe, Wis.

Greenhouses:
Cor. Farnsworth Ave. and Union St.

Cut Blooms of
Choice Water Lilies in their Season

Edmund D. Sturtevant,

The Water Lily Garden.

Bordentown, New Jersey, U. S. A., Feb 4th 1889

Prof Lorenz Watson

Dear Sir

In the plant catalogue of Reasoner Bros of Manatee. Florida there is a description of the palm "Attalea Cohune" which is credited to you. I wish to obtain some fresh seeds or nuts of this palm for propagation and venture to ask if you can direct me how and where to obtain them. By so doing you will greatly oblige me,

Respectfully Yours

E D Sturtevant

DR. E. LEWIS STURTEVANT.

So. Framingham, Mass.,

Jan. 25 1887

Prof. Lewis Watson,

Dear Sir:

Last summer, at Hampton, Mass., insect injuries about the base were quite frequent in Solidago simplicimus, so we can scarcely assume that the gall like excrescence produced the previous year & the variety (?) sent you. However, this coming autumn I will endeavor to collect you a series of specimens. In the first specimen in my own herbarium the whole plant (in bud) stood 11½ in. by ¼ in, by 10 in. tall. another (in flower) measures 17 in. tall, and was probably 11 in. by 3/8 in. as it stood, for I did not measure it at the time of collection.

Very truly Yours

E. Lewis Sturtevant

South Framingham,
Mass.

Dec. 12. 1888

Will Mrs. Asa Gray please receive
sincere thanks for the Copy of the Writings
of Dr. Asa Gray. Not alone the eminence
of Dr. Gray, but the slight personal
acquaintance, makes the book a most
desirable addition to my Library.

Respectfully

Elmo Hartwell

South Framingham,
Mass.

Oct. 1. 1890

Prof. Sereno Watson,

Thanks for copy of Contributions to American Botany XVII, just received.

Very truly
yours
Chas Thurber

Dr. E. Lewis Sturtevant.

So. Framingham, Mass.,

Oct. 11 1887

Prof. Irene Watson
My dear Sir;

I would like much to see your
plums this year, but I cannot leave
home. My wife was taken with puerperal
mania Dec. 31. last, and since then I
have had to keep close at home.

I wish I could trespass sufficiently
on your patience to give you a clear
[illegible] image. You have
[illegible] (two) pair grafts, [illegible],
[illegible], [illegible], and two [illegible] Watson each
[illegible] a series [illegible] variations, in grafts
2, by and C. Watson there a distinct resemblance
to hybridization or cross fertilization. The "wild
image" belongs to each group noted above, a
very few [illegible] plants from the characters
of my domesticated varieties, although I have
yet some valuable problems to be solved: hence,
the wild is just coming in condition to
be a case where Dr. Watson's work for me
applies.

I send you a series of pictures to
come. We can see you [illegible] Mr. Lyon, and
later we hope to [illegible] the city.

May see a [illegible] my address entitled "A Study of Negro" in the N.J. Ag. Ex. Sta. Report for 1884. [illegible] Island Sparrow I presented to the National Museum.

Very truly Yours

[illegible] Sturtevant

Dr. E. Lewis Sturtevant.

So. Framingham, Mass.,

Oct. 6 1880

Dr. E. Lewis Sturtevant.

So. Framingham, Mass.,

Dec. 28 1880

Professor Lewis Watson,
Dear Sir:

Ayotli is the Nahua generic name for gourd, equivalent to the Spanish Calabaza, as Dr. Brinton informs me.

In Hernandez mex. 1651, an described Ayotectli, by same Chichicayotli, (which seems to answer to Cucurbita radicans, Cogn.); and Ilallayotli, this last with woolly seeds ("lanuginoso semine")

Can you suggest any cucurbit with woolly seed, or which might have been so described by a 17th century botanist?

Can you furnish me with any modern vernacular name for Cucurbita species now used in Mexico?

If I am trespassing too hard on your patience in making these inquiries, please frankly decline, and I shall not be offended.

Very truly Yours
Lewis Sturtevant

I am preparing a paper entitled "Cucurbita an American Genus". I have laboriously compared all the figures of the old writers, and have prepared a table which may interest you, and hence I copy it on the next page.

Rumphii Herbarium

Editions	Total no. of varieties figured	Number of varieties that are new.
1511. Herbus Sanitatis	1	1
1542. Fuchsius	2	2
1554. Dodonaeus	3	1
1560. Matthiolus	1	1
1576. Lobel	4	4
1586. Camerarius Epit.	1	1
1587. Lugduni	6	1
1590. Tabernaemontanus	14	12
1591. Lobel	7	2 ?
1613. Eystettensis	3	3
1617. Castor Durante	1	1
1640. Parkinson	6	1
1648. Marcgrave	1	1
1650. J. Bauhin	19	6
1673. Cancrinus	2	2
1680. Morison	12	5
1688. Rheede	1	1
1719. Tournefort	2	2
1744. Morandi	1	1 ?
1747. Rumphius	1	1
1751. Gesner opera	1	1
	89	50

Varieties mean different figures which are evidently not copies of each other, and which show some difference.

Editions	Number of varieties figured, not new.
1546. Bock	2
1550. Rosselyn	2
1552. Tragus	2
1561. Pinaeus	1
1583. Dodonaeus	6
1597 Gerarde	13
1609. Uffenbach	1
1629. Parkinson	1
1636. Gerarde	10
1644 Bodaeus	3
1658. Piso	1
1666. Chabraeus	19
1678. Verzascha	1
1696. Zwingerus	2
1737. Lonicerus	2
1753. Dalechampius	6
	72

Gerarde's Herbal, 1636 has nearly entirely different figures from the Herbal of 1597.
Bock is German; Tragus is Latin edition.
Lugduni is Latin; Dalechamp. is French edition.
Tab. ic. 1590 figures are all in Tab. Kruyt. 1591 and 1613.

E. Ernst Trimbach
Dec. 28. 1890.

Dr. E. Lewis Sturtevant.

So. Framingham, Mass.,

Dec. 31 1880

Prof. S. Watson,
Dear Sir;

Thanks for your letter.

In 1883, W. S. Allen of Chocaluska, Fla. a competent observer, yet not a botanist, wrote me that the "Common Indian Pumpkin" of the Seminoles & the Everglades was a perennial, but the Cushaw was annual as at the north. I am disposed to put faith in Mr. Allens statement — and yet the only recorded Cucurbita as a cultivated perennial is C. ficifolia.

I think it probable that the "Citron Watermelon" (considered American by Vilmorin) in some cases has been C. ficifolia. I suspect the Chinese watermelon & the San Francisco Chinese is C. ficifolia, but the fruit far different from Naudin's & Cogniaux's description.

Mere suspicions I am trying to turn into certainties one way or the other.

This letter does not require an answer, unless you have material that

uncl and me. I just have a respect for a busy man, and dislike to trouble unnecessarily.

Very truly yours

Elmer Sturtevant

Perhaps you can name to me the address of some botanist who is at has collected in Southern Florida.

DR. E. LEWIS STURTEVANT.

So. Framingham, Mass.,

Jan. 28 1891

Rev. Walsh

Dear Sir:

Mr. Brandegee has looked up the "watermelon" of China-town, San Francisco, for me. He brought a slice of one. In external appearance a watermelon: interior white, and the seeds light-colored. The seeds and placentæ did not completely fill the interior.

This description suggests in part *Benincasa hispida*: in part does not obtain. He sent me a few seeds, and I enclose some to you, hoping that you may grow them. If a *Cucurbita*, it must be a new species or variety. This "watermelon" is eaten cooked, "like a squash."

Very truly Yours

E. Lewis Sturtevant

So. Hadley, Mass.

June 1/91

Last spring I found a small specimen of
Stipa Richardsonii, about here. The specimen
was determined by Prof. W. J. Beal. If he is
correct (I had myself named it Oryzopsis Canadensis,
but I am not an authority) this is a very
southern station.

Very truly Yours
Elmer Merriam

Dr. E. Lewis Sturtevant.

So. Framingham, Mass.,

Aug. 22 1881

Dear Sir:

Please accept thanks for copy of "Contributions to American Botany XVIII" and accompanying "abstract". I value these publications and appreciate the courtesy.

Very truly Yours
E. Lewis Sturtevant

Dr. E. Lewis Sturtevant.

So. Framingham, Mass.,

July 7 1886

Prof. Serno Watson,

Dear Sir:

I send you by express copies of the six reports of the N.Y. Ag. Ex. Sta. which under my charge. Please have these bound strongly in one volume, and have the bill sent to me. I desire much to have these reports on your shelves, as I think there is some work therein which is deserving. I would call your attention to germinations, 1884. 118 and 1885, 84.

The corn articles 1884, 124; 1885, 95 and 1886, 58. Especially the last. p. 60, and wherein an entirely new * species of corn is figured; but as it did not reach me, the seed was lost.

If it would add any interest to your paper in preparation, I would gladly send you a list of all the ancient figures of corn in my library.

Very truly yours

E. Lewis Sturtevant

Sturtevant

White Salmon, Wash. Feb. 18, 1881.

Rec'd Mar. 10/ + list rec'd
81 Sept. 3

Prof. Sereno. Watson.

Dear. Sir.:

During this last summer I have picked up several hundred plants, which I ~~send~~ send to You to-day. Not that I ~~suppose~~ suppose they may be of some value to You, but because it will be of some value to me to know the true names of these plants. If You will be kind enough to make all needful corrections on my list, I shall be very much obliged.

I have not yet seen any yellow-flowered specimens of Senecio lugens here, but east of the Klickitat river the flowers are bright yellow.

The change takes place a mile or two west of that river, and seems to be very sudden.

Suksdorfia is also on the Klickitat river near its mouth.

Has Calochortus nitidus with lilac fl. been collected at other localities? The stems of this one always has a little bulb on one side, an inch or two above the old bulb.

I remain

Very Respectfully
Yours
W. N. Suksdorf.

White Salmon, Wash. July 29. 1881

Prof. Sereno Watson.

Dear Sir:

In the little collection of plants, which I sent to You last spring, there are some small specimens of a fourth North American gayophytum, which is certainly a new species if it is not found in Chili. I found larger plants of it in Falcon Valley or Camas Prairie in June. It differs very much from the other species, which are much branched above, but perhaps never below. In this plant the branches (if any at all) come from axils of the cotyledons, and sometime also from 1 or 2 of the next leaves. The branches are simple and leafless

below for some distance. The capsules are very much compressed, hardly thicker than the leaves, many-seeded. It is about a month earlier than the others and grows (in the valley) on lower grounds at the base of a dry hill. 5 or 6 inches may be the maximum hight.

The other plant, Hemophila, is perhaps related to N. parviflora but the pubescence softer, stems more erect and much shorter, and seeds larger. Is it N. breviflora, Gray?

The ripe fruit of Prunus emarginata var. mollis (of which I sent some flowers last spring) is not black here, but red; or is it an other specie? I enclose a few stones of the fruit.

I am making collections for distribution this season.

Very Respectfully Yours

W. N. Suksdorf.

Ans^d Sept. 3^d

White Salmon, Wash. Aug. 20. 81

Mr. Sereno Watson

Dear Sir:

Last spring I found a very peculiar kind of clover, which I thought was new. I sent it to Dr. Parry with a few others plants, and named it Trifolium barbatum. In an answer he advised me to send it to you with the proposed name. I do so now and enclose also some others.

No. 1. Ranunculus multifidus. Borders of ponds. Columbia River, Klickatat Co., Wash. July 1881. The large scales of the petals are shaped somewhat like the rays of Dahlia and the like: the edges of the scale meet at base to form a sort of tube or border around a pit, while the top is rounded, truncate, emarginate, or even two-cleft: petioles pubescent.

No 5. Gilia minima, Gray — Wet level ground. Yakima Valley Klickatat Co., Wash. July, 1881 Small flowers white.

No 6. Gilia divaricata, Gray — Dry hill sides. Yakima Valley Klickatat Co. Wash., July 17th 1881. Small; branches leafless below: flowers purplish.

No 7. Echinospermum hispidum, Gray — Flowers Moist shady rocks, Columbia River. Klickatat Co., Wash. June 1881. Flowers white. Perennial, 1 or 2 feet high.

No. 8. Eleocharis acicularis — Springy places and meadows, Klickatat Co. Wash. July 1881 Annual an inch or 2 high.

Very Respectfully Yours

W. N. Suksdorf

When under water the leaves are more finely divided. The scales look like a smaller set of petals.

No. 2. Arabis ---- Level places on mountains near White Salmon River, Wash. May, fruit in July, 1881. Perennial, ½ to a foot high or more: the white flowers and pods are reflexed.

No. 3. Trifolium ~~so~~ stratum n. sp. I think it is very much T. ciliatum. Stems 6 to 12 inches high: leaves oblong, the earlier sometimes emarginate; tubes of the calyx lanceolate very unequal, the lower one short and nearly straight, the two upper longer than the corolla and curved outward especially in fruit: flowers purple, mostly in 2-4 whorls: ovary 2-ovuled: pods 1-seeded, rarely 2-seeded: peduncle continued beyond the flowers into a beak-like appendage, which is 4-7 lines long, green, smooth and slightly curved near the tip. I found it in June on moist grounds on the Columbia River Wash.

No. 4. Rigiopappus leptoclados ~~Centrocarpha carota~~. Dry hill-sides on the Columbia River, Klickatat Co, Wash. May 1881. Each scale of the involucre subtends a flower, and holds its akene long after the disk-akenes have dropped to the ground. The scales are somewhat in two series, the inner ones having rayless flowers and do not hold their akenes as firm as the outer ones, or not at all. These facts misled me, and I looked for it under Layia and it is, until I sent a specimen to Dr. Parry who takes it for P. exilis. But it seems to differ somewhat from the typical form. The scales of the involucre are green and not smooth: the pappus consists of 3-5 scales which gradually taper from the base to the top: the akenes appear as if transversely wrinkled. Small heads are sometimes rayless.

White Salmon, Wash. Ter.
Sep. 14. 1881

Sereno Watson Esq.

Dear Sir:

Please excuse me if I write again to ask a favor. Will you have time to name some 50 plants for me, besides those which I sent last spring? If not, will you please give me the adress of some one, who will do it? Since I am making collections for sale I am able to send good specimens now. Hoping, that I have not asked too much, I remain

Very Respectfully Yours

W. N. Suksdorf

will try to name them, or if he
prefers he can send them to Hon. Edwin

are smaller and paler. —

No. 63 Camassia — is indeed the common camas here, largely collected by Indians, and that is the only reason why I took it for C. esculenta. The flowers are plainly irregular and the petices seem to be always short and erect. It is very common from the Columbia R. to near Mt. Adams.

Did I shall be very thankfull if You will tell me under what names to send out several other plants, of which I sent specimens before: 1. A white-flowered aster from Mt. Adams. — 2. Eriogonum pyrolaefolium, n. 3. Aster radulinus, var? = No 395 of my last collection — 4. Pentstemon confertus, No 460, a glaucous form with glandular panicle, growing rarely in open timber. — 5. Sisyrinchium stellaris has petals with a heart-shaped base, like S. leucanthemifolia. —

Bidens frondosa is introduced. Rigiopappus leptoclados has yellow rays here. Very Respectfully Yours W. N.

My post Office and Land
Agency
White Salmon, Wash. T. Oct. 26. 1881

Dear Sir:

Your letter of Sept. 2. with corrected list of names, came here in due time. Please accept my thanks. After reading Your card, which I received some days ago, I venture to send an other small collection of 66 plants. I shall be exceedingly glad to get them named. But if you have no time to name them for me, will you please direct the package to some person, who will have time to do it? In that case I will send the necessary postage to You. In this package there are several plants, which were sent once before but there was some doubt about them, mostly of course, because the specimens were to bad.

No. 15 is the same as 372 of my last collection. It seems to me, it differs very much from 374 or Epilobium [illegible] in every respect, from the root to the seed. It grows on meadows sometimes, which are dry in summer, and when the plant is pulled up, the little buds remain in the ground very often and therefore it looks like an annual plant.

I send some gayophytums again this time, for I do not know under what names to send out the different forms. No. 20 and 21 were sent to Dr. Gray some years ago as G. racemosum and G. diffusum, but No. 19 does not belong to either of them, I think. 20 is often one-sided, and 21 has its flowers (which are large) more in little clusters in the axils of the leaves and at the base of branches. Otherwise the two are much alike. Seeds ½ line long, somewhat clavate or obovate, grayish with dark dots. No. 19 has

smaller seeds [illegible], oblong, yellowish. No. 2[illegible] 20 and perhaps also of 21 fo[illegible]ms mixed with 19. — No. [illegible]ry glaucous leaves and very [illegible] The goldenrods (Nos. 28, 29, 30) are probably all the same. 28 has rough leaves. 29 is lower, has smooth leaves, larger heads and broader rays. 30 is from a higher locality and has a narrow, racemi[illegible], straight panicle. — No. 56 was named Polygonum Persicaria in my last list of names P. Persicaria is found here also, and so I may have sent [illegible] plant last spring. This one (56) was intended [illegible]. It is an indigenous plant no doubt, very frequent on low [illegible] lands near the Columbia [illegible]; perennial, the prostrate stems rooting at the joints, the flowering portion mostly ascending. — No. 62 is [illegible], Brodiaea Howellii (61), but the flowers

No. 15 is the same as 372 of my last collection. It seems to me, it differs very much from 374 or Epilobium [illegible] in every respect, from the root to the seed. It grows on meadows sometimes, which are dry in summer, and when the plant is pulled up, the little buds remain in the ground very often and therefore it looks like an annual plant.

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No. 1. Cremastra ligantifolia !
2. odratia Lyallii !
3. Listyrianum ~~finetum~~ linifolium
4. Silene Menziesii !
5. Silene douglasii, Hook
6. Lupinus albicaulis, Dougl.
7. Lupinus [illegible]
8. Trifolium gracilentum
9. Trifolium cyathiferum
10. Rubus pedatus, Sm
11. Saxifraga bronchialis, L
12. Trifolium grandiflorum, Dougl!
13. Stellaria Jafida, Lindl., var.
14. Hedysarum cylindricum, Dougl. var. alpina, mass.
15. Epilobium [illegible] — mountain 3/4
16. Epilobium [illegible] !
17. Epilobium [illegible], Gray
18. Epilobium minutum, Lindl
19. [illegible] 709.
20. Saxifraga [illegible] 1 stock only — from Q.M., also from Carlt.
21. Saxifraga diversum, 709.
22. Saxifraga [illegible] — mostly the pubescent form (S. micro-

45 Stachys palustris Scorleri

46 Stachys betonica, Benth?

47 Betonica officinalis ×

48 Pentstemon digitalis? smaller form

49 Pentstemon glandulosus ×

50 Mimulus rubellus, Gray

51 Mimulus alsinoides, Dougl.

52 Castilleia parviflora. ×

53 Castilleia miniata, Dougl.?!

54 Scutellaria angustifolia ×

55 Stachys palustris, var. ×

56 Polygonum hydropiperoides, Mx.? but branches not ciliate

57 Epilobium coloratum

58 Urtica Lyallii

59 Gratiola virginica, Linn

60 Allium acuminatum, Hook

61 Brodiaea Howellii

62 Iris " ; mallow ?

? Camassia

~~64 Campanula [illegible]~~ Campanula rotundifolia

~~65 Veratrum [illegible]~~

~~66 Sedum [illegible]~~

~~67 Ranunculus [illegible]~~ Ranunculus repens

Some doubtful plants not figured

1. Arabis caucasica, hort
2. Eriogonum pyrolaefolium, var
3. Aster radulinus var.
4. Pentstemon confertus
5. Saxifraga (363) Stellaris

White Salmon Klickitat Co.
Washington T., Oct 30 '84

Dr Asa Gray

Dear Sir:

With this letter I mail 2 packages of botanical specimens. There are over 200 different plants, of which I have seen the greater number for the first time. A report on this collection will therefore be very interesting and of great value to me. Last spring I had a chance to ride from here to E. Washington. It was not a botanical trip; but I picked up some things, that I saw near the trails and roads. Some specimens are of course of little or no value.

I have discribed 3 plants as new species, but would not

over

publish them unless I received your approval. If not there is nothing lost. I shall certainly follow your advise.

The first of these species is a perennial clover with involucrate heads (near T. involucratum and T. tridentatum, I believe; the second whorl of flowers in the heads of T. trid. has an involucre of its own sometimes)

The second is a Valerianella, some of the forms of V. macrocera. I fear you will think me obstinate for touching this matter every year. But the more I study these forms the more I become convinced, that there are really two species. I have now several forms of each (one from California), but there are probably many more. I presume you are the only person who can decide, which of the two should be V. macrocera?

For the other, V. Grayi would be a very appropiate name, and it would be a pleasure to me to show my regard in this way.

The third plant described has every thing required to be a Pentstemon, except that the leaves are alternate. I collected the specimens at 2 stations some miles apart; the arrangement is alike in all.

I was prevented from sending my specimens sooner.

I am

sincerely yours

W. N. Suksdorf.

White Salmon, Wash. T.
Dec. 6, 1884

Sereno Watson, Esq.

Dear Sir;

Your letter with report on plants was duly received. I am very thankful! very thankful indeed!

The Ferula (no 325) was sent 5 or 6 years ago, in flower, and later in fruit. But the specimens were very poor. It was referred to F. multifida, under which name I have distributed it. I have never seen F. multifida if this purple-flowered plant is another species.

And the one clover is T. involucratum? I was surprised at such a mistake, but ~~mostly~~ hardly

disappointed because it did not prove to be a new species. I thought of *T. involucratum* at first; but I had no specimens to compare with mine. Finding nothing which would indicate, that the well known species was sometimes perennial I felt pretty sure, I had a different plant. This shows that one cannot be too careful. It may seem that I have a great desire to make new species; but I think that is not the case.

The two valuable books have come to hand, and I have sent a money order to Dr. Gray.

Sincerely Yours

W. N. Suksdorf

P.S. If you decide upon the name of the purple *Ferula* soon, will you kindly drop me a card? W. S.

White Salmon, Nov. 10, 1885

Prof. Sereno Watson.

Dear Sir

Many thanks for your report on some of my plants. I was unable to write sooner. For, my parents died a short time ago.

I send herewith another package of plants to Dr. Gray, and hope you will kindly correct a part of my determinations.

If you have heard from Kew in regard to the sedums, will you kindly let me know the result? I ask this favor because I have both species in my new sets.

Very truly yours

W. N. Suksdorf.

White Salmon, Feb. 18th
'86

Dr. Asa Gray

Dear Sir.

Your card of the 8th inst, which tells me of the Ranunculus Suksdorfii, reached me yester-day. I was very much pleased and sincerely thank you for this honor! But I am sorry to say I have no specimens of this plant with mature fruit. I have a single specimen with one head of perhaps full-grown akenes, which I send herewith. I shall visit this plant again next summer, if I can. It is a pretty Ranunculus, the flowers being large and almost orange-yellow.

Sincerely yours

W. N. Suksdorf.

White Salmon W. T.

June 7th 1886

Asa Gray

Dear Sir,

I received your recent Botanical Contribution a short time ago. I am very much obliged to you for your kindness.

When botanizing the other day I collected Salmiea. The flowers of this plant are described as having 5 petals. But I cannot find a fifth petal. I find 5 calyx lobes 4 petals, 3 stamens and 2 styles, and the calyx tube seems to be open on one side from the first.

Very truly yours

W. F. Suksdorf.

[illegible]

Grantham Lincolnshire
1866

To the Revd, Cambridge.

Dear Sir.

You will, I trust, pardon a stranger troubling you with a few lines on behalf of Mr W^m Lakeland, in whom I have been very good as to show a very kindly interest. He has an intense love for our knowledge, and has acquired, entirely unassisted, very much knowledge of it, do you not think he should be where he could have facilities for making that knowledge available and of course increasing more? He lives very isolated, having no intercourse with scientists, or, indeed, with any one of your genius tastes or interests. Is there any place to him means the centres of scientific culture, where is more advanced more rapidly scientifically, it is of

financially? He possesses a genuine scientific nature, thoroughly unworldly and indifferent to all pecuniary considerations, studious, industrious and conscientious in principal and in work. He has talent in drawing and sketching, and would of one time be become an artist. Could this not be made available in botanical work? He is extremely modest, and a very humble place would satisfy him. He has little or no means. He earns a bare livelihood making collections, which of course would cease should he leave his present home.

He is a child of nature, with some of the polish acquired by friction with society; but grief and unobtrusive, kind hearted, willing and obliging, and, tho' extremely unlemonsha-ble, appreciative of kindness.

As you doubtless know, he is of German birth. His good mother, to whom he was tenderly devoted, died some months since, forsome within forty-eight hours by his father, be buried in one grave.

William and his six brothers, (of whom my husband is one), have obtained more or less education by their own exertions, my husband having acquired a profession in that way.

Pardon my mentioning this bit of family history, that you might know somewhat more of William's circumstances, and not of at his request. I thought, however, that there might be some place in the botanical garden, museum or elsewhere, for which he would be fitted, and where his faithful, conscientious work might secure advancement.

Very respectfully yours,

Mrs H. F. Lockslop.

349 West Park St.

Portland.

Oregon.

White Salmon July 1st 1886.

Dr. Asa Gray.

Dear Sir,

My sister-in-law of Portland has just sent me your reply to her letter which she wrote for my sake. It shows again your readiness to help whenever it can be done. It makes me sad to be unable to accept your offer. An opportunity like this I have never had before, and for a while I did not know what to do. But I think you will agree with me after I tell you the reason why I can not go to Cambridge. My life so far has been a life in the open air rather than indoor

life. I am pretty sure that a sedentary or indoor life would not agree with my health very well. I might get used to such a life, but that is very doubtful, and I think it will be best not to risk it. I am fully aware of the advantages of such a place as you offer, and you can not know how I am tempted to accept it. It is hard for me to disappoint Mrs. Suksdorf who has always taken so much interest in my affairs; but better now than later.

I will answer a few of your questions although they have lost their importance now. I am 35 years old, and read German about as well as English.

I really believe I am better off in the field than elsewhere, for my fears in regard to indoor life are not unfounded. And my prospects here are better now than ~~they~~ what they have been.

Very sincerely yours

W. N. Suksdorf.

White Salmon W. T.
July 31st '86

Dr. Asa Gray.

Dear Sir,

I am just back from the high mountains. Have been absent for about 3 weeks, and now find two valuable letters from you which should have been answered long ago. I am sorry I was not here to reply at once. I read your letters with great interest and they made me happy. You still offer me a place and I almost feared that my refusal might offend you. For it was nearly like asking a favor and then

refusing it. Indeed your kindness is unbounded!

You want me to come to Cambridge and try it for a year. In that case I gladly accept your kind offer. I shall do all I can to keep in good health, but I make no plans yet for the future beyond the first year.

Will you please let me know at what date I must be there at Cambridge? If possible, I should like to finish my work of this season which would take a few months. If I have time enough to distribute my plants collected this season, I will send you a set for determination (also the Delphiniums that I have) soon after I receive your reply; if not, I shall take along a set when I go myself.

I remain

Your much obliged

W. N. Suksdorf

White Salmon W. T. Aug 5th '86

Dear Sir

Your favor of July 22. was duly received. I am sorry to say that I have paid no attention to our Ontaria this season and therefore I have nothing to add this time. I have more specimens of the undetermined Draba from Mount Baldo, but no ripe fruit yet. I found another Cardamine this spring, also a small white Draba. But I cannot find a description for them.

Yours sincerely

W. N. Suksdorf

Amity P.O., Scott Co., Iowa.
March 2, 1889.

Dr. G. L. Goodale,

Dear Sir,

You will please excuse if I write to you in regard to my plants, which are still at the Harvard herbarium. Perhaps I ought to write to Mr. Watson. If that is the case, please let me know it, and I will write to him. It is sometimes very hard to avoid mistakes of this sort. I would ask you to return my plants to Wash. T. Addressed to: Philipp Suksdorf, White Salmon, Washington T. I will have to pay

2c 4½

freight in advance, I think. I shall send the money as soon as I know the amount of all expenses.

I hope, Dr., that you and your family are all well, give my regards to Dr. Farlow.

My little school closes in a few weeks; then I shall go back to W. T., no doubt.

Yours very truly

W. N. Suksdorf.

Spangle, Spokane Co., W. T.
May 7, 1889.

Dr. Sereno Watson,

Dear Sir

Your letter of Mar. 28th was duly received. I have since made the journey from Davenport, Iowa to this place. I am very much obliged to you for packing and shipping my plants. I was unable to do that myself when I left Cambridge last summer. It would have saved me and others a great deal of expenses and trouble if the plants had been kept at home. But I did not know that, when I started. I think the mosses are of little or no value.

Still, I should like to have them. Please send the package to White Salmon Washington T. I will return postage in a letter. With kind regards to all I remain

Yours truly

W. N. Suksdorf.
Spangle,
W. T.

White Salmon W. T.
Oct. 16, 89.

Dr. Watson.

Dear Sir,

I returned to White Salmon a few days ago. I have examined my plants and thank you for the care with which they were packed. In your letter you mentioned a package of mosses, and the last time I was in Cambridge I left a bundle of papers or other things in the Herbarium. Will you please let me know how much it will cost to send them to this place? — I would like to distribute some plants this fall. I send you some 23 plants. Will you kindly correct and complete my work in naming them? The Pentstemon bothers me as also the Gentianas, Psilocarphus has broader leaves than my specimens of P. Oregonus from the Cascade

mountains. I think Cusick has collected the Crataegus ~~sp~~ before but I do not remember the name. The rays of n 933 are probably too large for Bidens cernua. Has the yellow, fragrant Cypripedium (C. parviflorum?) been collected before in the N. W.? I have some more plants (a few specimens of each kind) which I will send later.

With kind regards

Yours Truly

W. N. Suksdorf.

932. Pa[illegible]

924. Aster [illegible]

929. " [illegible]

930 " [illegible]

925. " [illegible]

926. Solidago [illegible]

927. B[illegible]

923. [illegible]

922. [illegible]

Sukdorf-

White Salmon, Washington
Oct. 11, 1890

Dear Sir,

A few days ago I received your Contributions No. XVII., which you kindly sent me. I am much obliged to you. I want to buy the new Manual, if possible. Will you kindly inform me where I can get it and what the price is? Have you any copies of your Contributions Nos. XV and XVI? I would like to have these, if possible, buy them if need be. I intend to send you some plants that I have collected this season. I have been collecting on the Sound for

a short time but have not been very successful.

Yours truly
W. N. Sukosdorf.

Cross-sections of the fruit of.

Valerianella Grayi et macrocera

V. congesta

V. samolifolia.

Valerianella Grayi. Corolla nearly white or pale rose-colored; the 5 equal lobes spreading; spur thick, straight, as long as the body; stamens mostly exserted; fruit glabrous or sometimes puberulent; the broad face between the wings not marked by a prominent midnerve, often densely pubescent; the larger leaves sometimes incisely toothed or lobed at base.

Closely related to V. macrocera, Gray, which has a somewhat bilabiate corolla, with its short lobes erect or nearly so; spur mostly long and then curved downward; stamens usually shorter than the corolla; fruit marked by a ridge or prominent midnerve between the wings; the smooth and shining outer membrane on each side of this nerve is separated from the body and wing, making the latter bilamellate at base; the two cavities thus formed within the wings are sometimes rather large.

Columbus Ohio May 15th 1873

Prof. Gray

Dear Sir

My brother made no disposition of his herbarium and numerous bundles of specimens of mosses nor of his Bryological library – during the winter he dictated a few memoranda concerning the plates of his works & a copy of which I will send you as soon as I can get it, containing as it does some requests for you –

He rec'd one day while I was there 3 sets of proofs for his supplement and almost the only expression of impatience and escaped him during his sickness was on this occasion when he expressed his regret that he was losing time and he so much wished to be at work, saying it would take him but 3 or 4 weeks to finish it – Yet we have not found descriptions although his wife says she wrote out quite

(including his bryological books) it would be in consonance with my own views and feelings and I believe also of that of his family, but of this I cannot now speak positively –

My brother left to our Agricultural College a valuable collection of general botanical works – and I believe if we had appointed any one who was really capable of taking charge of the botanical department he would have given much more not feeling we were quite in the condition to raise that department to its full stature nor having any applicant I thought quite qualified I advised against any appointment and a sort of smattering for it has been consigned to Townshend our Ag Prof who from his position as member of ours Board was forced upon us – & in fact and wants to be President forced himself – In due time next year I hope the chair will be properly filled – As I am charged with the duty of equipping the college with apparatus for those departments needing it

a number from his dictation — I understand the engraving and the plate paper in the hands of the engraver has been paid for — A set of the proofs he put in the hands of Schroeder his draughtsman for correction and we are anxious that work shall be completed under your advice & supervision if you will be kind enough to undertake it — I hope still to find his own descriptions so far as he wrote any — but if not, must depend upon some of his finds and as to this I wish your advice — Mr Lesquereux kindly offers to do so if required — But — inter nos — My brother said once he thought I did not write quite as good Latin as he could wish — he also expressed the opinion that C F Austen of Chester R I. was one of the best Bryologists in the country — but I could not undertake the task unless [illegible] and consequently the collection must be kept here if he is to do it — Meeting with a good opportunity to sell the house it has been

[illegible] and the collection will have to be removed at once — I propose to place every thing upon cataloguing in a room in our medical college — I infer from a conversation with my brother a good while ago that he desired his bryological collection should be kept together and I think in case of his death would have liked it placed in your charge with the ultimate destination of your own collection to it (Harvard College?) He said to me early in the winter, if there was any institution out here where he was sure it would be cared for he said he would like to give it to them but he knew of none — and I think the idea of placing it in your care for publication and preservation was always uppermost in his mind — I have understood Harvard had or was about erecting a fire proof building for the preservation of your botanical collections — If so and you would take charge of my brothers valuable collection to be kept together for publication under proper regulations

5th

I will be very much obliged if so soon as your convenience will permit you will tell me what means of illustration we will need in botony in stable physiology &c and where I can obtain them – The Board has placed $25000 – in my hands to be expended by me for apparatus &c &c – If not too much trouble give me your own outfit and course of instruction – where does it come in or should it come in connection with our Ag– College – 1st 2. 3. 4th year and how long continue &c in short whatever you can suggest from your own experience will be of value to us – We hope to open this fall with a well equipped Chemical and Physical laboratories and reasonable in the other departments needing apparatus &c Having no faith in a purely agricultural school I have endeavored to create also a scientific school of high character with a good prospect of success in that direction – very truly yours J Sullivant

Columbus Ohio Oct 28th 1873

Prof Gray

Dear Sir

On my return from my visit to my brother in Illinois where I very much enjoyed the fine air of the Prairies, I found your letter — Mr Lequereux is at work and I trust in due time to submit his labors to your criticism — He says if it is desirable, he will send his work and a set of the plates to Lindberg for his corrections as he has very kindly offered his assistance which I trust will not be needed although I believe there is no one whom my brother would have more willingly entrusted with his unfinished works as he had great confidence in his skill and scholarship When Lequereux is through I will send all the books and collections to Cambridge for preservation — provided the collection be kept distinct and not distributed and incorporated with others — very respectfully Yours

J Hellins

Columbus Ohio
Nov 24th 1873

Prof Gray
Dear Sir

My hand is enfeebled with rheumatism which confines me to write with a pencil as it is easier for me than a pen — Mr Leggett's visit to the West and the necessity of writing this report stopped the commencement of his work on the Icones II — Longer than either of us anticipated, and it was not until my return from Illinois late in Oct that he commenced I saw him this morning and he says he is getting along well — he thinks one third of the descriptions are written so that with your kind assistance he may soon get to finish the last work of my brother whom I so sorely miss every day. When Sister Carrie left in July she promised me to call and see you and I presume she did so — She was very

send them all to you to dispose of as you saw fit — but said if they were not disposed he thought a set of specimens might be had by writing to you — I asked him to do so but I presume he may have forgotten it — Now if it is not asking too much I would very much like some specimens for our College — especially the botanical part of it — if botany is to be taught — I feel a little anxious about this whole matter and would like to see it satisfactorily settled before I leave the Board, for as Columbus now has little to keep me since Wm's death I think I shall leave it in the spring and join my dear brother, the big hearted farmer on the Prairies of Illinois

Respectfully yours
J. Sullivant

publication to insist that the Horticulturist should be kept distinct and I am glad to find from your letter such is your wish as well as ours and that there will be no difficulty in the way — as to the books, they are packed for you and with a label indicating their source might go into use before you receive — I will send every thing except those specifically disposed of by time — when the proper time comes —

And now about our Ag College — It is very probable the chair of Botany may be filled at our meeting January 3d — Persons candidates for the place have been and may be proposed — as chairman of Committee on Faculty I cannot receive any of the applicants — How would Austin or Bebb do? They are the best I now think of — as a Western man Bebb would stand the best chance — A few weeks since a Doctor from Painesville Ohio called on me to get a copy of my brothers catalogue of the Franklin Co plants — I lent him my own copy — I cannot now recall his name but he spoke as if he had recently seen you, and from all this you may perhaps remember him — he seemed to know a good deal of Ohio botany and is more interested in majors — How would he answer? As to Sereno Watson to whom botany was new till I am amazed he should undertake it — This I was compelled to do to prevent a worse appointment at that time And rather than see sick of a poor appointment to the full chair would rather leave it where it is — But must be prepared to present some name if it becomes necessary — so that whatever information you can give me will be thankfully received and used discreetly — entirely confidential if desired — and for the sake of the Science I hope you will communicate as soon as convenient —

A few months before my brothers death I asked him to donate to our College some of his Rhenopterous plants — particularly Ohio and Western — he replied he would have done so, but that he had

Cleveland Ohio March 11th 1874

Wm Gray

Dear Sir

I was out this morning when
Mr Seymour called and did not have the
pleasure to meet him but he left your
note for him — Two or three days after receiving your
note I handed up the two numbers of the Lincoln
Journal and made them to your address 150 in
Lincoln Cambridge Mass and hope you have received
them before this — Your allusion to the various
notices which may have troubled me — Schrader
claimed that my brother had given him the
collection to dispose of for his private benefit
for which he made drawings and sketches of the
new ones — which he says he did and I believe he
had the right to — but he also claimed my brothers
agreeable manner which he did not (does so)
and consequently the collection was of no use
to him and then claimed the set with the
drawings which I refused him — And he is

very much put out about it – I told Bro
Livingstone I would be guided by what you
and he would determine to be right – Of course
I would withhold nothing from Schroder that
properly belonged to him – but I would also feel
bound to look to the rights and reputation of my
dear brethren. The members of the Elon Brigade in
your county direct to my care for the Elon College
Catalogue will be sent which I will see found – It was
our conviction the College would continue the
subscription – and pay for the numbers in your care
now that it passed to them more to the
Col yet as Sister Carrie desired to give them to me
but I thought the above disposition would be
more in accordance with my brethren wishes –

Yours Truly
J. Sullivan

Columbus Ohio
July 31st 1874

Prof Gray

Dear Sir

Mr Lesquereux having informed me had completed the manuscript for the new Icones and proceeded to pack and forward to your address the Herbarium and Bryological library of my dear brother — to be preserved in the Harvard fire proof building and with the understanding this Herbarium is not to be broken up and distributed among other herbaria but is to be kept together as Sullivant collection — Could not pay charges further than Cleveland, tried to pay Cambridge — please pay and add to account of Mrs S or send bill to me Hope they will arrive safely — I will write again soon — respectfully yours

Sullivant

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Columbus Ohio
August 31st 1874

Prof Gray

Dear Sir

I am glad to learn from your letter that the 2d Icones progresses so well and trust it may soon be brought to a completion satisfactory and creditable to all parties

I respectfully suggest you strike out from your memoir of Torrey the words "so called" in connection with the Ohio University as being unmeaning and uncalled for and calculated to give offence - for among the alumni of that Institution are many who were and are of national reputation - with this amendment I think it very desirable it should be printed in the vol - indeed if it can be done without delaying it I am very much disposed to put in an engraved portrait if I know an engraver to be trusted for rendering the likeness - but will not delay the publication for this purpose - I shall be glad to receive for our college any duplicate books from his collection - I am anxious to get an authentic and full collection of Cyperus Carex and Gramineae from my brothers old collection or any other if it is to be had - and if you already have in collection Austins Appalachian &c I would be glad if that sent was returned if you recall it and I will add it to Alleghanicus and Ruricolous and give them to our college - indeed I wish to deposit here as complete a collection of my brothers specimens and works as can now be had and must rely on your kindness for assistance - Trusting you and Mr Sargeant will succeed with the Icones to your own satisfaction and approval we shall all be obliged for this labor of love towards the just reputation of a dear kinsman respectfully & truly yours

J. Sullivant

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Columbus Ohio
Septr 8 1874

Prof Gray

Dear Sir

It is perhaps a small matter the expulsion" so called" to but the fact is, it still exists although in a very languishing condition but at the time spoken of it was a State institution and the most promising and important of any West of the Alleghenies, and certainly turned out some very distinguished men and among them friends of myself and brother. His getting there was more difficult and longer than now to go to Cambridge. The journey was on horseback with attendant aid a pack horse to carry the baggage. It was an evidence also of the strong desire of our father to secure for him the then best educational advantages. But enough on this a matter of taste.

I rejoice to learn my brothers work is to see the light under good auspices and I desire that full acknowledgement shall be made for the valuable assistance rendered. Sister Carrie has always insisted that she copied the descriptions for the supplement and that they were all written sometime before by my brother but I have not found them in the papers I have examined. Supposing you had a copy of the "Insecta Exercitata" of Lesquereux & Sullivant I retained a copy for his family if they want it, otherwise for our College. But if it is needed for his collection I will send it. So also as to Austins work. I thought if you already had a copy you would not need this, would not otherwise have asked for it. So of the Cyperaceae and Gramineae I know they are not very necessary in his Phaenogamous herborium for 9

helped collect them – but they would be valuable to us as collected in this vicinity – Indeed I would be glad to procure for our College a full and authentic set from somewhere –

I like the idea of a photographic book especially as I have already some experience in that direction having had copied one in the book I send you to buy by Express charges paid – It is too heavily printed but form a good deal to write I think very good – the standing figure is good and the last my brother had taken January 1872 – pretty good of Michael and myself except the position of the last, stiff awkward and constrained being jammed into a corner – If you have a better card than the one different size I send please send it me and I will return it – I always intended to offer a copy of my unpretending book for your acceptance and waited to have one better bound with the plates in it but I send one in ordinary binding having interculated the plates as well as could be done –

Some of these times I hope to see a competent and worthy successor in our College expounding and illustrating the science my brother and myself loved – until then – patience and charity for pretense, hoping the future may bring forth something better –

I may say I originally only intended a slight family sketch of Sullivant only – When my brother Irwin read my sketch of our father he insisted I should go on and take in others and that he and Michael would publish it – At Xmas he wrote for him to come in and keep up the Old Virginia and family customs as our father before us always did – And at that meeting he expected with him to arrange for the publication – He could not come until in January and then he was prostrated by his final sickness – I only mention this to illustrate a trait in my brothers character –

Accept the book then not for any merit therein but as evidence of personal esteem

my truly yours

M. Sullivant

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HENRY S. BABBITT, Columbus, Treasurer.

Columbus Ohio
Jan 21st 187[illegible]

Prof. Gray

Dear Sir

An attack of illness has prevented a sooner reply to yours of the 14th and even now it must be brief.

I desire that you use your own judgment and discretion as to binding such [illegible] of Rural, and as to the remaining copies of Vol 1. I would think a suitable number of each should be put on the market - but on all this matter the exercise of your own judgment will be satisfactory and approved by us

Yours truly
Sullivant

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Columbus Ohio
Febr 28th 1875

Prof. Gray

Dear Sir

Ill health has prevented me from replying to yours of the 19th sooner — In relation to the distribution of 2d vol of Icones I told Prof. Lesquereux I would leave it to you and him to distribute gratuitously (as the wish of the family — my own especially) to all friends and correspondents as would most likely have been done by my brother — I think I so wrote you — We do not wish to make any money out of the work only hoping it might pay expenses — If I have not heretofore made myself understood — I will repeat — that I expected you and kind Lesquereux to take charge of my dear brothers work edit print and distribute according to your judgment having full discretion in the whole matter — do whatsoever you think for the best and it will be entirely satisfactory to me — and I represent the family — I desire you should have one copy bound in same style as my brothers presentation copy to you and charge to general expense — which copy is to be considered from the family — This is not to prevent you from taking as many copies as you wish otherwise —

I certainly have no objection to place some copies at disposal but would much rather rely upon your and Lesquereux discretion than leave as to who should have presentation copies I desired to have 1st & 2 vols bound in good style for Lesquereux but he says with his usual modesty and disinterestedness that he only wants 2d vol in cloth — I very much desire

that he has some copies placed at his disposal – I want 2d vol in cloth to match first vol which my brother gave me – one copy of 1st and second vols to place along with others of his botanical library he gave to Ohio Ag College and one of 2d vol in stitched not bound – but which I will have bound to match his presentation copy to Ohio State Library and finally 3 copies 1st + 2d Vol for Mrs Rodgers my brother daughter Eliza –

Recapitulation 3 copies (one in sheets) vol 2 –
4 copies 1st + 2d vols –
and if Lequereux will let me a well bound set 1st + 2d for himself –

All else concerning the Icones please do as you think best –

The unusually cold winter has been one trying on me and my health is not good and am sorry to think has also probably told on yourself – I hope however you are better and likewise will be benefitted by the coming summer shine very truly yours

J Sullivant

Columbus Ohio
April 27th 1878

Prof Gray
Dear Sir
Mr Rogers called
last evening with your letter
about the Flora Brasiliensis &c.
When I received your note and
enclosure I took them to the College
and gave them to Prof Townshend
who promised to make the affidavit
and return to you immediately—
I will go out on Monday morning and
attend to it myself and send you a check
for the amount due— Please inform
me how many numbers yet to complete
the work? Our college is being reorgan-
ized or rather Democratized with all
other State institutions— I expect in a
few days to give place to a political
brummer, and it is doubtful if the

new powers will carry out the
agreement to continue the subscription
to complete the work. Carrie S
desired me to take the mss as they were
left by my brother, but I proposed
with her consent to turn them over
to the college and made it a condition
that it should continue the subscription -
but reserving the right if the college
refused to become owner ship by
me paying or refunding the advances
made by the College for the mss paid
for by it - We have never had a
bolt or rivet to whom they would be of any
use and are now not likely to have -
Our institution has been one of the
best equipped and most promising of any
in the West but as it is now to become
a pawn in politics and a fountain
of reward for the merest partizan services
I fear its day of usefulness & prosperity is
over - Very respectfully yours

J Sullivant

University Press.

(Established 1639.)

WELCH, BIGELOW, AND COMPANY,

Stereotypers, Electrotypers, Printers, and Binders.

Cambridge, May 12th 1875.

Prof. Asa Gray,

Dear Sir!

We sent You the Zoner Supplement as follows:—

88 copies folded mtr. plates packed

123 " " " without " " .

100 " Bound

4 " folded mtr. plates sent by Express

2 " " " " " "

2 " " without " " "

319 copies in all delivered Jan—

May 2nd

Welch, Bigelow & Co

per C. J. Little

P.S. We have all ready to go on with Botany of California as soon as Copy is furnished

TRÜBNER & Co
AMERICAN, EUROPEAN AND ORIENTAL LITERARY AGENCY.

57 & 59, Ludgate Hill,
London Dec 31 1877
E.C.

D: Asa Gray
Cambridge, Mass.

Dear Sir,

We have the pleasure to inform you that we have requested our bankers Messrs. Brown Shipley & Co to pay you the equivalent of £9.12.0 the balance due to you as per statement on other side which we hope you will find correct

Your acknowledgement of same will oblige

Yours truly
Trübner & Co

62
1876
Jan 1 By Goods on hand as per account rendered Dec 31/75 £14.8.0
" 8 " " 2 Silliman's Scenes @ 3/- 6.--
2 Supplement to do @ 2/- 4.--
10.--
Less ⅕ 2.--
8.0.0
22.8.0

Dr
1877
Dec 31 To Stock on Land
2 Scenes 4.16.--
2 Supplement 3.4.--
2 Manuel 4.16.--
12.16.0
£ 9.12.0

Memoranda for my Executors.

In the North West attic room of my house are three (or four?) boxes, some two feet three inches, by eighteen inches, by five inches, containing 129 engraved Copperplates, being the original Plates of my Icones Muscorum.

In the same room also are four boxes, some five feet long, each containing 50 impressions of those Copperplates - that is to say 200 impressions, and sufficient to make 200 Complete Volumes.

In the same room - part in a Bookcase and part in a square box - are from 30 to 40 copies, complete, in folio form, of the Mosses of Wilkes' Exploring Expedition. - All the above are in that North-west Attic room.

There is also at Cambridge, in charge of Dr Asa Gray, letter-press for 200 complete Volumes of the Icones Muscorum.

W. H. Dougal (of Georgetown, D.C.) has just completed - and is now having struck off - 200 Copies, each of 81 plates, of the Supplement 1. to my Icones Muscorum.

Some eight or nine years ago I left with Dr. Asa Gray 8 engraved Octavo plates, illustrating the Genera of Musci and Hepaticae of his Flora of the Middle States; - a letter recently received from him expresses an apprehension that those plates were des-

troyed in the Boston fire.— this however is to be ascertained.

Now all the above-mentioned property.— Plates, Books, Impressions, Letter-press &c. including moneys paid to draughtsmen, have cost me at a low estimate $15,000 or $16,000.— I desire that my dear old friend, Dr Asa Gray, should sell all the above things on the best terms he can, and, after paying all expenses and himself twelve per cent for his trouble, pay over the remainder to my Administrators.

Mark 45/3

THE

Am. Ex. Co.

TRANSACT A GENERAL

Express Business between all the Principal Cities and Towns of

NEW YORK,
KENTUCKY,
WISCONSIN,
WEST'N PENNSYLVANIA,
MICHIGAN,
MISSOURI,
OHIO,
ILLINOIS,
MINNESOTA,
INDIANA,
IOWA,
CANADA.
NEW ENGLAND STATES,

And connecting with other responsible Expresses to

All Parts of the World.

AMERICAN EXPRESS COMPANY.

(60

COLUMBUS, Ohio, June 14 1873

Received of A. D. Rogers - Ex'r

9 Bxs said to contain Books & Plates

valued at ____ Dollars, 100

Marked Prof Asa Gray Cambridge Mass

Which we undertake to forward to the nearest point of destination reached by this Company, subject expressly to the following conditions, namely: This Company is not to be held liable for any loss or damage except as forwarders only, nor for any loss or damage by fire, by the dangers of navigation, by the act of God or of the enemies of the Government, the restraints of Government, mobs, riots, insurrections, pirates, or from or by reason of any of the hazards or dangers incident to a state of war. Nor shall this Company be liable for any default or negligence of any person, corporation or association to whom the above described property shall or may be delivered by this Company, for the performance of any act or duty in respect thereto, at any place or point off the established routes or lines run by this Company, and any such person, corporation or association is not to be regarded, deemed or taken to be the agent of this Company for any such purpose, but on the contrary, such person, corporation or association shall be deemed and taken to be the agent of the person, corporation or association from whom this Company received the property above described. It being understood that this Company relies upon the various Railroad and Steamboat lines of the country for its means of forwarding property delivered to it to be forwarded, it is agreed that it shall not be liable for any damage to said property caused by the detention of any train of cars or of any steamboat upon which said property shall be placed for transportation; nor by the neglect or refusal of any Railroad Company or Steamboat to receive and forward the said property.

Nor shall this Company be liable for any loss or damage of any box, package or bale, for over fifty dollars the just and true value thereof is hereby stated; nor upon any property of any kind, unless properly packed and secured for transportation; nor upon any fragile articles, unless so marked upon the package containing the same; nor upon any articles consisting of, or contained in, glass. This Company will not be liable for any loss or damage unless the claim therefor shall be made in writing within thirty days from the accruing of the cause of action in a statement to which this receipt shall be attached. The party accepting this receipt hereby agrees to the conditions herein contained.

For the Company, A. Dunn Agent.

Memo. of Boxes rec.d from J. Sullivant,
June 17th 1873.

× 3 Boxes, containing 129 Copper-plates of the "Icones Muscorum."

×× 4 " of impressions from the plates, each box supposed to contain 50 imp.s.

××× 1 " containing 23 bound copies of the "Music of the Wilkes' Exp.n Exp.n", and some loose sheets.

×××× 1 " containing "Taylor Herbarium".

Upstairs, left end of Sullivant Cases

× (Two in Locked Case Dr. [illegible] S. E. Cor.)

××× Upstairs on shelves. " " " " "

×× Up Stairs, in Laboratory

×××× Numbered.

The 51 copper plates of the Supplement are in 2 Boxes in the lower locked cupboards in the main Herbarium room.

S.I.W.

The copper plates of the Music & Hepaticae of the Manual are in the upper locked cupboards. July 1879

Copy from Account of Railway with Estate of W. S. Sullivant.

(Last settled March 30, 1876)

Estate Dr.

March 25, 1876	To paid postages	.25
" 1 1877.	To paid charges on copies of Jews Music. delivered to Nichol. Leipsic. & presentation copies —	6. 00
Sept. 21, 1876.	To paid for Telegraphic dispatches for Ohio State University —	$15. 00
July 11, 1877.	To Ditto ..	33 60
	charges	1.61 51. 21
1878. Feb. 1.	To check of Brown Bros,	47. 04

Cr.

1876		
April 4.	By Cash for sales of Jews	8. 00
June 28.	" "	27. 20
July 22.	" "	19. 20
(1877)		
Oct. 9.	By Cash for Music & pd. exp'd.	9. 00
" "	from Nichol. for Jews Music.	54. 40
April 10.	" from Cassino. " "	30. 00
Dec. 31..	Copy of Music. & pd. exp'd. sent to DeCandolle, Geneva —	9. 00
1878. Jany. 22.	Cash from Trustees for sales	47. 04.
		203. 84
Feb. 1..	By Draft on Ohio University	51. 21
		$255. 05

47.04
51.21
6.25
104.50

203.84
51.21
9.34

Dr Asa Gray

57 & 59, Ludgate Hill, London,

D.r (21/442)

in account with Tribner & Co.

C.r

1879				1878			
May 31	Stock on hand			Jan 1	By Goods on hand		
	1 Sullivants Icones 11.66				2 Sullivants Icones 23.32		
	1 Suppt to do 7.76				2 Suppt to do 15.52		
	1 Sullivants Musci 11.66				2 Sullivants Musci 23.32		
		31	08			62	16
				Feb 25	By 4 Synoptical Flora of N. America	18	00
	Balanced down	49	08				
		80	16			80	16
				1879 June 1	Balance down $	49	08

Columbus O. July 23/88

Mr. Lorenzo Watson
Cambridge Mass.

Dear Sir.. Your note in reference to certain books, plates &c which the late Prof Gray had charge for the Estate of the late Mr. S. Belmont dec'd was received some days ago.

I handed the note to Mr. Allen Belmont who is now attending to matters in connection with the Estate of his Father, also replied to, and requested him to correspond with you about this property. If he has not already done herein probably address you soon. In the meantime do as you in your own judgment dictate in the premises of them or others coming for them.

Very respectfully yours
A. D. Rogers

It may be well to correspond with Mr. Edward Burgess who I presume you know.

Aug 1884

Bound Copies on hand —

In Herb. —	Leaves	1
	Suppt.	2
With Weigel	Leaves	2
	Suppt.	2
May/79 Mubmorda	Leaves	1
	Suppt	1

THE COLUMBUS
CONSOLIDATED STREET R. R.
COMPANY.

A. D. RODGERS, PRESIDENT.

E. K. STEWART, SECRETARY.

COLUMBUS, O., March 16 1891

Mr. Jerome Watson

Bellaire Gardner Cambridge Ohio.

Dear Sir. Yours of 2nd inst. received. The matter to which you call my attention I was reminded of when Dr. Gray first began making Order for the Settlement of Mr. S. for his Executorship, and requested him (Dr. G.) to obtain the Consideration for his trouble which was then suggested, but I am aware he never did so.

Although I have virtually turned over the management of the Estate of Mr. S. to the Heirs under an arrangement to pay them certain sums as I could have them [illegible] I am aware I reserved the direction of many items connected with it & must ask you to retain the full amount of Compensation above stated for Mr. S. in the memorandum referred to. I suppose you will do so not only as to this fund but in your pending Order.

Dr. Gray's services to the family of his old friend were in this particular of value from the knowledge of the management & care of the Estate. With kind remembrances to Mrs Gray & yourself

Very truly Yours

A. D. Rodgers. Trustee

THE COLUMBUS
CONSOLIDATED STREET R. R.
COMPANY.

A. D. RODGERS, PRESIDENT.

E. K. STEWART, SECRETARY.

COLUMBUS, O. May 18th 1891

Mr. Sereno Watson
Botanic Gardens Cambridge Mass.

My dear Sir: Yours of 8th inst. enclosing check on Charles River Nat'l Bank for $156.71 proceeds of sales of Bronze of the late W. S. Sullivant dec'd, was mislaid at the time of its receipt else it would have been acknowledged at an earlier date.

Thanks for your attention to the matter. In the future I hope you will retain the Commissions as sales are made.

Yours truly
A. D. Rodgers Sec. Trustees

Senate Chamber
26th Jan. '60

My dear Professor,

Pray keep me informed with regard to that missing 10th vol. I feel the loss of a work of science like that as a personal grief.

If you have it I can lend you the whole set of the Pac. R. R. Rep'ts except Vol

I am poorly provided with the 2nd vol. of Emmy's Resp. Of the 1st vol. I had 70 copies of the 2nd 12 copies only. I hope to send you one of these; for my desire is to distribute these where they will do the most good.

Believe me, my dear sir,
Yours faithfully,
Chas. Schieber

C. W. SWAN, M. D.,
32 Worcester St.
OFFICE HOUR, 3-4 P. M.

Boston, Sept 21 1884.

S. Watson, Esq.

Dear Sir: With this I send by mail in separate package a Nicotiana cultivated (Boston). The flowers are sweet-scented & said to be vespertine. The corolla exceeds in length that of any described in the N. A. Flora and otherwise does not seem to answer to any one of the descriptions. Some of the lower leaves are decurrent. The plant is fully three feet high. Will you kindly name it for me?

Very truly Yours

C. W. Swan

For identification of Dillenius'
Mosses — see Arnott Tilton in Hook.
Journ. Bot. 1, 66 (1834.)
Lindberg

C. W. SWAN, M. D.,
32 Worcester St.
OFFICE HOUR, 3-4 P. M.

Boston, Dec. 17 1885.

Prof A. Gray, Cambridge, Mass.

Dear Sir: Taking the Manual as a guide I wish to state what you probably already know very well, that Lonicera Tatarica grows in Maine. I saw it on the Kennebunkport side of the Kennebunk river near the so-called Landing, in considerable abundance on the low hills along the river, and sparingly on the Kennebunk side opposite, on grounds belonging to Mr. Kingsbury, who pointed out the plant to me & stated that he had observed it for three or four years past.

Please do not take the trouble to acknowledge this note.

Very respectfully

Chas W. Swan

C. W. SWAN, M. D.,
32 Worcester St.
OFFICE HOUR, 3–4 P. M.

Boston, March 7 1890.

Dear Sir: The enclosed Stellaria — as I have called it — was collected as you will see by the package, which contains all that remains after mounting for the herbarium, else I would have been more generous.

I should be much obliged if you would kindly identify it for me — at your entire leisure & convenience. It does not seem to have a place in the Manual.

Very truly yours,

Chas W. Swan

DEPARTMENT OF
NATURAL HISTORY
AND
GEOLOGY.
GOODWIN D. SWEZEY, DIRECTOR.

Doane College.

Crete, Neb., Jan. 2. 1889

Prof. Sereno Watson:

Dear Sir;-

I take the liberty of sending you, with request to name, our common species of Cynopterus here in Nebraska. What is it?

Yours truly
Goodwin D Swezey.

DEPARTMENT OF
NATURAL HISTORY
AND
GEOLOGY.
GOODWIN D. SWEZEY, DIRECTOR.

Doane College.

Crete, Neb., Nov. 15, 1891

Dr. Sereno Watson,

Dear Sir:- I take the liberty of sending you four plants which I should be very grateful if you will give me the names of. Three of them as published in my "Nebraska Flowering Plants" were new to the state but I am now in doubt as to the correctness of my identifications.

The Aster is a common one here but is it A. Tradescanti. L, as I have supposed?

That other composite was named for me as it is labelled, by one in whose opinion I had a good deal of confidence; but can it be

Swezey

DEPARTMENT OF
NATURAL HISTORY
AND
GEOLOGY.
GOODWIN D. SWEZEY, DIRECTOR.

Doane College.

Crete, Neb., 188

a coreopsis at all, with so retrorsely bristly awns? Th. Pentstemon was thought by the same authority to be an unusually pubescent P. caerulens, Nutt. (or is it P. albidus?) The Chenopodium I have supposed to be C. Boscianum or is it C. leptophyllum?

Perhaps the material I send you is rather meager but I have been obliged to divide herbarium specimens in order to keep duplicates; and I imagine you will find no difficulty in identifying them.

Very truly yours,

G. D. Swezey.